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STUDIES OF GERMAN YOUTH *

BY DONALD V. McGRANAHAN, MAJOR, AUS, AND MORRIS JANOWITZ, 2ND LT., AUS
Bad Homburg, Germany

INTRODUCTION

IN order to study the residues of Nazi education among German youth, four samples of German boys and girls between the ages of 14–18 were given write-in questionnaires in late July 1945 by representatives of the Intelligence Section Information Control Division, U.S.F.E.T. Two of the samples represented a rough cross-section of the youth of these age groups in middle-sized communities in the western zone of occupation. In one town, Friedberg, 135 children (59 boys and 76 girls) were assembled in a schoolroom by summons of the Burgermeister, who drew the boys and girls from all parts of the town. In the other community, Offenbach, the youth (140 boys and 116 girls) were selected at random while they registered for the re-opening of the German educational system in that city. Religious affiliations were fairly representative for Germany as a whole. No marked differences in attitudes were encountered between the Friedberg and Offenbach samples.

In addition, two special samples were studied for comparative purposes; one a random sample of 285 young prisoners of war, mostly aged 16–17, who were undergoing a re-educational program in an American prisoner of war enclosure in Northern France; the other a small group of girls (aged 14–18) who were courageously trained in an anti-Nazi tradition in a private school during the Hitler regime.

* The material in this article has already been released for publication, and has appeared in summarized form in various newspapers.

The questionnaire was administered by U. S. personnel who followed a standardized procedure. They explained that the test was being conducted because they were interested in the opinions of young boys and girls in Germany, and the youth should take the opportunity to express their opinion freely and without restraint (*ganz frei und ohne jeden Druck*). The youth were not asked for their names and were reassured that their identity would be protected. In addition, standardized instructions were given on filling out the answers.

The validity of information derived from anonymous questionnaires administered by U. S. personnel in Germany may be called into question. There is no doubt that on a number of topics the youth answered with an eye cocked on the American eagle. The results, particularly on the more direct questions, must therefore be viewed with this fact in mind. However, it must also be remembered that the Allied occupation is a dominant and real fact in the psychological environment of every German. In order to understand and predict German youth behavior, it is necessary to record systematically the attitudes displayed in this environment. Such attitudes are no less "real" and significant than the "private attitudes" which may be expressed only in secret to German friends, but which, where they exist, do not appear to have great firmness and force today.

If, in anonymous questionnaires, the youth are deferential and servile to Americans, and if defiant Naziism is rare, then it is fairly safe to assume

that similar sentiments will appear in other manifestations of behavior. Conclusions from these youth studies, revealing a submissive and confused state of mind, are quite consistent with other information on the current behavior of German youth. General Eisenhower has publicly referred to the absence of "Werewolf" activity. In view of the current conditions of broken family life, food and fuel shortages, and lack of schooling, it is surprising, indeed, that there is not more unruliness among German adolescents, on purely non-political grounds.

SUMMARY AND CONCLUSIONS

The majority of the youth displayed pro-democratic and pro-American sentiments, and rejected obvious Nazi symbols. At the same time, however, when certain basic values came into play, and when the youth apparently did not know the appropriate democratic pass-word, their Nazi background became apparent, thus creating a general picture of inconsistency and confusion. The girls appeared less pro-American and more Nazified than the boys. No significant differences could be found between the attitudes of younger and older children.

On the basis of the two studies in the communities of Friedberg and Offenbach, a hypothetical picture of the "average" German civilian youth under American occupation can be drawn up in way of summary. This youth wants to continue living in Germany, and considers Russia the worst place in the world in which to live. He feels that the Germans as a people are superior to the Italians, Poles, Russians, and French but not to the Americans and English. The officers were justified in attempting the Putsch of 20 July, 1944, he feels, because it was clear that the war was lost at that time. It was lost primarily because of the material superiority of the Allies, especially in air power. He is ready to admit that Germany started the war, although he is apt to add that the Jews or Englishmen or Poles were also responsible. He agrees that the German Jew should be permitted to return to Germany, and that Hitler Youth and BDM leaders should be prevented from taking leading positions in any new youth movement. As for women, their place is still in the home, not in outside careers.

This youth expresses the view that Germany should now become a democracy. At the same time, however, he says that the idea of National Socialism was good but it was not carried out right. He hesitates on the question as to whether Hitler himself was bad or whether it was just his advisers who were bad. In any case, he feels quite strongly that Germany now needs a strong Fuehrer in order to recover from her destruction and devastation. He agrees whole-heartedly that a free press is a fine thing for a country but at the same time feels that newspapers should only print what is good for the people. Furthermore, he is strongly convinced that the average person is stupid and easily misled by propaganda. He will not commit himself to saying that Germany has been misunderstood and unjustly criticized, but he does not firmly deny it. Roosevelt is the individual whom he singles out as the greatest man in history, with Bismarck and Frederick the Great as close second choices. He indicates very little concern for political matters. His chief concern is getting enough to eat. After that he worries about his own personal future in the matter of education and job.

Results of these studies suggest that the major problem of German youth today is not the danger of persistence of the Nazi movement among them,

with attendant resistance and defiance of the Allied authorities. German youth have lost their old leadership, and the great majority appear to have little hope or desire to recover it. To a certain extent, Nazi leaders and symbols are being replaced by democratic or pro-American ones.

This process, however, should not be construed as evidence that these youth have changed their basic character and are now becoming young democrats—they are still totalitarian youth in search of leadership. They now echo what they consider to be the official views of their current masters. In a sense, the very manner in which they quickly pick up and express democratic and pro-American views reveals their totalitarian attitude of implicit and uncritical submission to authority, ingrained by Nazi education and German tradition. There is little evidence from other sources that German youth, including those intellectuals who proclaim the necessity of "democratic" propaganda, understand what democracy means as a way of life in ordinary day-to-day community affairs. The danger exists that adherence to democracy will be used more as a formula to expect assistance and sympathy from American authorities than as a formula for self-help and community action. Untrained in democratic processes, German youth appear to presume that the American leaders will now solve their problems in an authoritarian fashion just as the Nazi leaders proposed to do, without the youth having to do anything more than obey orders and profess adherence to the official creed.

ANTI-NAZI AND PRO-AMERICAN ATTITUDES

Perhaps the most striking indication of the overt rejection by German youth of Nazi philosophy can be found in their expressed attitude toward the Jews. On the question of the return of the Jews to Germany, the Friedberg and Offenbach youth in considerable majority disavowed the anti-Semitic policies so closely identified with the Nazi Government.

"SHOULD GERMAN JEWS BE PERMITTED TO RETURN TO GERMANY?"

(Percentages)

	BOYS	GIRLS	TOTAL
Yes	74	70	72
No	19	26	22
No answer	7	6	6

They also repudiated former Hitler youth and BDM (Bund Deutscher Mädchen) leaders.

"SHOULD HJ AND BDM LEADERS TAKE UP LEADING POSITIONS IN ANY NEW YOUTH MOVEMENTS?"

(Percentages)

	BOYS	GIRLS	TOTAL
Yes	22	15	19
No	69	81	74
No answer	9	4	7

The attempt made by the officers to overthrow the Hitler regime in July 1944 received wide approval from the youth.

"DO YOU THINK THAT THE OFFICERS WERE JUSTIFIED IN MAKING THE PUTSCH OF 20 JULY 1944?"

(Percentages)

	BOYS	GIRLS	TOTAL
Yes	81	67	74
No	12	22	17
No answer	8	11	9

Finally, the youth at Offenbach (the question was not given to the Friedberg

group) indicated general approval of "democracy" as the future form of government for Germany.

"SHOULD GERMANY BECOME A DEMOCRACY?"

(Percentages)

	BOYS	GIRLS	TOTAL
Agree	70	44	58
Disagree	14	22	17
No answer	16	34	25

The enthusiasm of the girls for democracy is notably less than that of the boys. The relatively high percentage of those who did not commit themselves on this question is also to be noted. Undoubtedly, some of those who disagreed or did not answer belonged to the fairly large class of anti-Nazi Germans who approve of democracy but are cynical about its possibilities in Germany.

U. S. A. as a country is complimented by German youth in their answer to the question: "In which country would you most like to live?" However, the number of German youth who confirm their desire to continue life in Germany is greater than one might perhaps expect. (Several double answers were given.)

"IN WHICH COUNTRY WOULD YOU MOST LIKE TO LIVE?"

	BOYS	GIRLS	TOTAL
Germany	144	171	315
U.S.A.	37	13	50
Switzerland	7	5	12
England	6	1	7
Africa	2	—	2
South America	1	—	1
Doesn't matter	2	—	2
No answer	5	5	10

Russia is the country where German youth definitely do not want to live.

"IN WHAT COUNTRY WOULD YOU LEAST LIKE TO LIVE?"

	BOYS	GIRLS	TOTAL
Russia	125	157	282
Poland	14	14	28
Germany	10	4	14
Africa	9	2	11
France	6	2	8
Japan	3	3	6
China	3	2	5
Italy	5	0	5
Any foreign country	2	0	2
U.S.A.	1	1	2
Others	4	2	6
Doesn't matter	5	2	7
No answer	15	8	23

NAZI RESIDUES

While the majority gave anti-Nazi answers to such questions as those given above, nearly all the youth revealed basic Nazi attitudes at one point or another. Those who gave anti-Nazi answers to one question would answer in a Nazi fashion to another, and vice versa. For example, only 21 out of the entire 391 gave consistently Nazi answers to three key questions (whether the officers' Putsch was justified; whether HJ and BDM leaders should be retained in future youth organizations; and whether German Jews should be allowed to return to Germany). Interrogators who have made more intensive personality studies have also remarked on this pattern of confusion and inconsistency.

It has been noted in other studies that many a Nazi-minded adult, when confronted by someone arguing the weak points of National Socialism, will resort to the counter-argument that the idea of National Socialism was good but it was not properly carried out. When the young Germans of Offenbach were queried on their opinions on this argument, the majority agreed with it.

"THE IDEA OF NATIONAL SOCIALISM WAS GOOD, BUT IT WAS NOT PROPERLY CARRIED OUT."

(Percentages)

	OFFENBACH BOYS	OFFENBACH GIRLS	OFFENBACH TOTAL
Agree	70	83	76
Disagree	27	15	21
No answer	3	2	3

Another typical argument of the Nazi-minded is that Adolf Hitler himself was not bad, but his advisers were bad. The answers given by the Offenbach youth to this question revealed a greater critical attitude toward Adolf Hitler than toward National Socialism as a whole. Nevertheless almost half accepted this particular myth.[1]

"HITLER WAS NOT BAD, HIS ADVISERS WERE BAD."

(Percentages)

	OFFENBACH BOYS	OFFENBACH GIRLS	OFFENBACH TOTAL
Agree	39	46	42
Disagree	54	43	49
No answer	7	11	9

When given an opportunity to express anonymously their attitude toward various nationalities, the majority of the German youth repudiated the Nazi theme of the social and moral inferiority of the American people. The English also came off well. The racial superiority doctrine, however, turned up in attitudes expressed toward the Italians, Russians, Poles, and French.

Simple questions of fact also revealed Nazi residues. Although the youth, for example, in majority admitted that Germany started the war, a significant minority held other parties responsible. Half as many as those who mentioned Germany referred to other groups, the most coming being the Jews. (Multiple answers to this question were common.)

"WHO STARTED THE WAR?"

	BOYS	GIRLS	TOTAL
Russia	4	5	9
England	16	12	28
Germany	143	138	281
U.S.A.	4	1	5
The Jews	26	25	51
Poland	26	13	39
No answer	22	18	40

More subtle indication of the mixture of Nazi values and pro-American or pro-democratic values appeared in answer to the question: "Who, in your opinion is the greatest man in the history of the world?" Roosevelt headed the list, closely followed by Bismarck

[1] Although these two questions are "double-barreled," a rough analysis of intercorrelations suggests that they are two of the most critical questions in differentiating general Nazi vs. anti-Nazi opinion.

"DO YOU THINK THE GERMANS AS A PEOPLE ARE BETTER THAN:"

(Percentages)

	BOYS			GIRLS			TOTAL		
	YES	NO	N.A.	YES	NO	N.A.	YES	NO	N.A.
The Italians?	75	17	8	66	22	12	70	20	10
The Americans?	26	59	15	27	44	29	27	52	21
The Russians?	59	31	10	70	18	12	64	25	11
The British?	35	49	16	28	42	30	32	45	23
The Polish?	73	17	10	70	18	12	71	18	11
The French?	58	32	10	56	26	18	57	29	14

and Frederick the Great. (Multiple answers were common.)

"WHO, IN YOUR OPINION, IS THE GREATEST MAN IN THE HISTORY OF THE WORLD?"

	Boys	Girls	Total
Roosevelt	45	18	63
Bismarck	19	31	50
Frederick the Great	17	32	49
Hitler	14	5	19
General Eisenhower	12	2	14
Stalin	8	6	14
Charlemagne	8	—	8
President Truman	6	2	8
"American"	—	7	7
Caesar	5	1	6
Alexander the Great	3	—	3
Napoleon	2	1	3
Christ	2	—	2
Columbus	1	1	2
Galileo	—	2	2
President of the U.S.A. (1 put "Fuehrer of U.S.A.")	2	—	2
Miscellaneous (boys—Churchill, Hindenburg, Heinrich VI, Great Elector, Pope; girls—Leo XII, Founder of 2nd Reich, Otto the Great, Karl V, Theodoric, General Stuben, Goethe, Bach)	5	8	13
Don't know, or no answer	53	77	130

In general, a large percentage of the children mentioned an American for their choice as the greatest man in history. A number of girls even felt it sufficient to write "American" in answer to the question. A very small percentage of the greatest men in history were Englishmen. (One would expect different results from the British Zone.) Nazi relics appeared in the expressed admiration of Roosevelt, in that several of the children misspelled his name, giving it a Jewish character, such as "Rosenfeld."

It is interesting to note that, while Hitler's name does not appear with great frequency, the particular pattern of choices clearly reflects a Nazi system of values. Bismarck and Frederick the Great were favored historical heroes of the Nazis and stressed in Nazi education. Hitler's personal study in the "Braunhaus" in Munich used to be full of pictures and busts of Frederick the Great. Perhaps even more significant, however, is the fact that with only a few exceptions, the individuals named are symbols of military or political power. Writers, artists, philosophers, scientists, inventors, discoverers, businessmen, and religious leaders receive but rare mention. For the German youth, "greatness" means "power." The fact that these German youth substituted Allied symbols of power like Roosevelt, Stalin, and Eisenhower for German ones indicates only a superficial change of values.[2]

ABSTRACT DEFINITIONS

The youth were asked to explain a number of generalized and abstract ideas. When asked to define what Democracy meant, the Friedberg group as a whole displayed striking inability to produce even the most simple stereotype association with the word.[3] Almost half either answered that they did not know or left the question blank. The girls were more Nazified or generally ignorant, in that 70 per cent of them made no attempt at definition, while 40 per cent of the boys failed in this respect. The definitions given were seldom more complicated than simple phrases such as "free opinion," "individual liberty," "equality of the people," "rule for the people," and the like. Examples of the fuller definitions encountered were:

[2] The fact that Hitler failed to achieve greater recognition is probably due to the current judgment of his failure. There is little evidence that among German youth a clear realization of the morality of Hitler's position exists today. Therefore these data should not be taken as ruling out the possibility that in years to come a myth about Hitler may develop among the present youth of Germany, on the basis of the goals which he sought to achieve.

[3] Results from the Offenbach youth on definitions have not yet been analyzed.

Democracy overthrows all racial laws with the declaration that all people are equal and thereby possess equal rights.

Freedom in thoughts, words and deeds insofar as one can answer for them before God and mankind.

Rule by the people; the people elect their candidates in a common free election, and no force is applied to them.

There were an isolated number of Nazi-inspired definitions, such as an "anti-German party which means the decline of Germany" or "a clash between two classes of people."

The definitions given for *race* proved valuable in revealing one of the most successful points of Nazi penetration. Well over one-third of the boys and half of the girls gave definitions which clearly showed the Nazi imprint. Such definitions involved phrases about the need for keeping one's race pure, racial superiority, and the like. For example:

A people is racially conditioned. I believe that a pure race has the best possibilities for existence. A people, whose blood is very mixed, will one day decline.

The Aryan race is better than the non-Aryan race.

It is the duty of each race to remain pure.

There are many races. Everyone should have a racial feeling in order not to join himself with other races.

A minority defined race in neutral terms, but gave no indication as to whether they accepted the significance of racial differences according to the Nazi point of view. Only an isolated youth offered definitions which clearly rejected Nazi training: "One does not need to consider them as inhuman because they have another color." In general, many more of these young people had something to say about race than about democracy.

INCONSISTENCIES

The following interrelations of data from the Offenbach study present particular examples of how the youth of Offenbach simultaneously accept and reject elements of National Socialism.

Those Who Rejected Former HJ and BDM Leaders. Superficially, the fact that 80 per cent of the sample voted against retaining former leaders of the HJ and BDM as leaders of any future organization might be taken as a measure of their anti-Nazi bias. However, of this group who rejected Hitler Youth leadership, (1) three-fourths still believed that Germany needs a strong Fuehrer; (2) almost one-third took the line that Hitler was not bad but that his advisers were bad; (3) over two-thirds persisted in the idea that National Socialism was a good idea, although not carried out properly. (4) On the other hand, among the minority (less than 20 per cent) who welcomed the prospect of continued leadership by Hitler Jugend and BDM functionaries, half rejected one of the basic Nazi arguments, namely, that Germany was misunderstood and falsely criticized by other countries.

Those Who Justified the Officers' Putsch of 20 July. Sixty per cent of the supporters of the Putsch, among the Offenbach group, still believed that Germany needs a strong leader. Twenty-five per cent of them continued to hold the position that the Jews should not be allowed to return to Germany. Conversely, of those (30 individuals) who opposed the Putsch, the majority believed that the Jews should be allowed to return to Germany.

Those Who Think Germany Should Become a Democracy. Fully 40 per cent of those who favored Germany's becoming a democracy still held on to the notion that National Socialism was a good idea but poorly executed. When

probed about some of the practices of democracy, the group revealed the full extent of the difficulties that lie ahead in their re-education. Overwhelmingly, they professed their belief in freedom of the press, but at the same time almost all of them thought that newspapers should be allowed to print only that which was good for the nation.

"NEWSPAPERS SHOULD ONLY PRINT WHAT IS GOOD FOR THE PEOPLE (VOLK)."

(Percentages)

Agree	77
Disagree	20
No answer	3

"A FREE PRESS IN WHICH EVERYONE CAN EXPRESS HIS PERSONAL OPINION IS A GOOD THING FOR A COUNTRY."

(Percentages)

Agree	84
Disagree	11
No answer	5

YOUTH'S PROBLEMS

A question was included in the Offenbach study to discover what the children consider to be their greatest current problem ("What causes you the greatest worry at the present time?") By far the most common answer was "food" ("Die Ernährung") or a remark such as, "My stomach is always empty." Eighty-nine children specifically referred to the food problem. The next most frequently mentioned concern of the boys was "jobs," while for the girls it was the problem of re-uniting with imprisoned, missing, or distant members of their families. Both boys and girls indicated considerable concern about getting back to school and obtaining an education. A fairly large number of boys and girls vaguely worried about their "personal futures" or about "the future of youth in Germany." A smaller number expressed a vague concern about "The future of Germany" or "reconstruction."

Other worries were expressed about living conditions in the coming winter and about coal and housing. The Russian bogey also turned up, sometimes in the form of a generalized fear of Russia or Communism, sometimes in the expressed anxiety that Russia would take over the American area of occupation. Several boys and girls said they were worried about the moral standards of German girls. One boy was most concerned over the fact that he had nothing to smoke and one girl was most concerned over the fact that "It was all in vain."

In general, the youth showed very little interest in the political future of Germany. They appeared almost entirely absorbed in personal and economic problems. Three boys did complain that Party members were still holding positions of influence, and one expressed concern that innocent National Socialists were being ousted too quickly. A solitary girl said that her greatest worry was "The shame of National Socialism upon Germany"; and a solitary boy was primarily worried as to "whether democracy would win" in Germany. Two girls mentioned the problem of religious freedom. The vast majority, however, cared more for food than freedom.

EXPERIMENT IN RE-EDUCATION

Nine thousand young German Ps/W, mostly aged 16 and 17, who have been collected at a prisoner of war enclosure in Northern France where they are participating in an extensive program of re-education, offered an opportunity to test the plasticity of attitudes among young Germans. Under the direction of an American educational authority, over 120 carefully screened older Ps/W with academic or religious backgrounds launched, in May, an extensive course

of study designed to teach basic classroom subjects, and to supply the basis for broader re-orientation.

Required courses include mathematics, science, English, German, and geography. Elective subjects include Russian, Spanish, French, higher mathematics, American history, commercial subjects, as well as philosophy and the history of literature. The study of English is one of the most popular subjects in the entire curriculum. Great stress is laid on the teaching of American history. The American supervisor of the program furnishes weekly the material for instruction in mimeographed form. Source material for the reports includes a number of standard historical works which have appeared in the Armed Services editions. Among them are: *The Republic,* Charles Beard; *Selected Writings of Abraham Lincoln; America,* Stephen Vincent Benét.

The aims of classroom instruction are supported by an elaborate extracurricular program. Dramatic art and choral work are the most popular, after sports. In addition to the regular camp religious program, formal classes of a sectarian nature are held. The young Ps/W have access to a library of about 400 volumes. Facilities are available for movies.

Random discussions with the young Ps/W reveal that they are aware of the object of the school and display no resentment because of it. Most striking was the complete absence of any feeling that the school was anti-German or some sort of plot which in one way or another would undermine their future existence.

All military procedures such as saluting and the like have been abolished. Military titles for the camp cadre have been changed to civilian-like titles. Most of the young Ps/W have been given an opportunity to sever one of their closest connections with the Wehrmacht, namely, their Wehrmacht uniforms. Instead, they now wear obsolete GI clothing, which is a source of great delight for many of them. In order to shake loose any "barbed wire" complex, these youths are allowed to take long hikes accompanied only by an unarmed American soldier.

Self-administration of justice and discipline has been introduced. The court, dealing with infraction of camp rules and composed of young Ps/W themselves, acts under the direction of a former non-Nazi of the Judge Advocate Department of the German Army.

Despite the energy which the youths show in constructing gravelled walks, sun dials, and camp theaters, and despite their interest in classroom activities, the educational administrator has found a powerful but erratic sense of defeatism among many. Everything in Germany is totally destroyed, some of them feel, and never again will be rebuilt. Others have lost their families and see little purpose in the future. It may well be that this spirit will be short-lived once these young boys regain their freedom; however, at the time of the study of their attitudes it was the basic problem. To combat it, the slogan "Life has the final say" has been accepted by the authorities as the central theme of their activities.

To probe the effect of the re-educational program, the same questionnaire which was administered to the civilian youth was given to a sample of 285 of the young prisoners of war, nearly all of whom were 16 or 17 years old. Representativeness of the sample was assured by drawing the pupils at random from those attending required courses. The German teachers were instructed in the administration of the questionnaire, and the group was given no special instructions other than those

mentioned above as being given to the other samples.

The result of the re-education can be seen in the fact that the Ps/W boys, as a group, consistently exhibited much stronger pro-American and anti-Nazi sentiments than their civilian contemporaries. Where the civilian youth gave a pro-democratic or pro-American answer in a fair majority, they answered in overwhelming majority (in several cases, 95 per cent or more); and in a number of cases where the civilian youth gave a Nazi response, they indicated a complete reversal of opinion. The brief military experience of the boys may account for some of the differences found, but the effect of the training is abundantly clear in specific instances.

However, the extreme pro-American and anti-Nazi views of these "re-educated" youth should not necessarily be construed as evidence of fundamental conversion to democracy. Compared with the civilian youth, they revealed a greater susceptibility to the influence of their new masters and a greater identification with the American side; but this may be due to their greater exposure to the American point of view. The durability of these views when the prisoners return to their homes and occupations is, of course, open to question. In general, the young prisoners displayed little critical and independent thinking, but much servility.

The difference in attitudes between the civilian youth and the prisoners of war can be seen in the answers to the questions given in the right-hand column of this page (for comparative purposes only the boys at Offenbach are used).

The prisoners of war were notably more complimentary to the Americans than the Offenbach boys in answer to the question: "Who, in your opinion, is the greatest man in the history of the world?" Roosevelt headed both lists, but in the prisoner of war group he received 36 per cent of all mentions, whereas at Offenbach he received 28 per cent. In general, 43 per cent of all candidates for historical greatness among the Offenbach boys were Americans, whereas 58 per cent among the Ps/W were Americans. Among the re-educated prisoners of war, the first ten names were: Roosevelt, Christ, Eisenhower, Truman, Churchill, George Washington, Napoleon, Stalin, Frederick the Great, and Charlemagne. An

Question	Percentage Saying "Yes"	
	Offenbach Boys	Ps/W
Do you believe the officers were justified in making the Putsch of 20 July, 1944?	77	96
Should German Jews be permitted to return to Germany?	76	95
Germany should become a democracy.	70	76
A free press in which everyone can express his personal opinion is a good thing for a country.	84	95
Newspapers should only print what is good for the people.	71	55
The idea of National Socialism was good but it was not carried out right.	70	25
Hitler himself was not bad, his advisers were bad.	39	17
Germany needs a strong new leader to recover from her downfall and destruction.	73	33
Should HJ and BDM leaders take leading positions in any new youth movements.	26	3
Other countries have misunderstood Germany and criticized her unjustly.	31	8
Do you believe that the Germans as a people ("Volk") are better than:		
The Americans?	30	7
The English?	43	9
The French?	61	50
The Russians?	65	47
The Poles?	72	59
The Italians?	78	63

outstanding difference was that Christ appeared second in the prisoner of war list, whilst barely appearing at all in the results of the Offenbach test. (Religion was given a prominent place in the training at the prisoner of war camp.)

The only place where the Ps/W boys displayed a view more consistent with Nazi propaganda was in answer to the question "Should girls take up a career other than home-making?" Eighty-three per cent of the Ps/W were opposed to careers for women, compared to only 60 per cent of the Offenbach boys. This difference may be due in part to more intense dreams of home among the young prisoners.

In answer to the question as to what their greatest worry was at the time, the young prisoners overwhelmingly indicated concern about their parents and relatives, and about returning home. Many wondered whether their parents were still living and others worried about the conditions they would find at home. Food, the most important concern of the civilian youth, was rarely mentioned by the young prisoners, although presumably implied in their worry about "conditions at home." Jobs and personal future figured fairly high. As at Offenbach, the youth revealed extremely little concern about political problems and the political future of Germany.

Similar to their comrades at home, about 75 per cent of the youth at the P/W enclosure selected Germany as the place where they would most like to live, with the U. S. A. as second choice (17 per cent). One P/W boy specifically designated "Texas," and another wanted most to live "Where there is plutocracy." Not one of the 699 German boys and girls thus far approached on this question has mentioned Russia as the country where he or she would most like to live. On the contrary, Russia is consistently and overwhelmingly the one place where German youth do not want to live.

A Group of Anti-Nazi Girls

In contrast to the young Ps/W who showed a lack of critical and independent thinking, a small group of girls who were brought up in a private school uniquely conducted along anti-Nazi lines during the Hitler regime presented a picture of more fundamental anti-Naziism. These girls, unlike the young Ps/W, showed little tendency to play up to the Americans and made a number of intelligent critical comments as, for example, that there were good and bad people in every country and it is character, not race, that counts. The majority of the re-educated prisoner of war youths still asserted German superiority over Italians, Poles, and French, whereas only two out of the 23 girls would agree to German superiority, as a general principle, to any other people.

In answering the question, "Who was the greatest man in world history?", two said Christ, two Caesar, two Bismarck and one Napoleon. The majority, however, stated that they were not in a position to judge. It is interesting to observe that these girls did not attempt to toady to the Americans by giving President Roosevelt's or President Truman's name. Their favorite country to live in was Switzerland, with France, Germany, and England following behind. Like the other children, however, these girls felt that Russia was the country they would least like to live in.

None of these girls expressed the opinion that the officers were unjustified in making the Putsch of 20 July, 1944. Only one felt that Hitler Youth and BDM leaders should take a leading part in any future youth organizations, and even this girl qualified her "Yes" by

saying, "Certain ones." All but one (the exception gave no answer) agreed that Germany started the war. Whereas two-thirds of the Friedberg youth could not even approximate an answer to the question, "Who was Heinrich Heine?" every one of these girls gave a correct answer, some explaining not only that he was a Jewish poet, but that he lived in Paris, was a romantic of the Nineteenth Century, and wrote "Die Lorelei."

ABNORMAL AND SUBNORMAL RIGIDITY*

BY HEINZ WERNER

Brooklyn College

RIGIDITY, here defined as lack of variability and adaptability,[1] has been observed as a normal trait of less developed organisms. The lower the organism, the more uniform the relation between animal and its world and, therefore, the more stereotyped the behavior. For instance, a frog, adapted to respond to moving objects, might well starve to death if surrounded by motionless flies.

Descending the ontogenetic scale, a similar increase of rigidity can be observed. Less variability exists in the immature than in the mature organism. To illustrate, a three-year-old child who has learned to use a round stick to pull objects toward him through a grating may have difficulty with the same task if the stick is replaced by a square one. More mature children encounter no difficulty when a slight change is made in the shape of the tool (15, p. 192).

Evidence is accumulating which indicates that rigidity is a behavior trait particularly characteristic of the subnormal mind. Goddard, as well as other early workers in the field of mental deficiency, insisted that lack of plasticity is an important factor in the difficulty displayed by feebleminded persons in their adjustment to the complex environment of our civilization. More recently, Lewin, Gottschaldt, and others have emphasized rigidity as a fundamental personality trait of the feebleminded (10).

Rigidity is also conspicuously present in abnormal behavior. Lack of variability has been observed in brain-injured organisms, animals as well as humans.

Neurologists are familiar with the peculiar "perseverations" of brain-injured patients. This clinical term was employed for the first time by Neisser. It indicates the persistent repetition or continuation of an activity once begun; for instance, perseveration may take the form of repetition of a specific word in the patient's writing or speaking. Gelb and Goldstein (4) concluded from their extensive work with brain-injured patients that rigidity is a general pathological trait that is present in these patients irrespective of the focus of lesion.

Experiments on operated animals have demonstrated an increase in rigidity due to cortical lesions. According to Cameron (2) and Buytendijk (1), brain-operated rats are less able than healthy animals to break a habit and form a new one. Norman Maier (11) reported that operated rats tend to perseverate, i.e., to repeat the same errors

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[1] Some authors, particularly students of Kurt Lewin, use the term "rigidity" in a predominantly structural rather than functional sense. They think in terms of "regions" of personality and conceive of rigidity as a dynamic property of a boundary which prevents communication between neighboring regions. Some of the disagreement in the interpretation of clinical and experimental work on rigidity seems to be due to this present confusion in the usage of the term (Goldstein, 5).

in successive trials. Hamilton and Ellis (6) also observed an increase of "behavior constancy" in the operated animals. Krechevsky (9) has indicated that, for the rat, relatively minor cortical lesions result in a loss of variability and plasticity of behavior. He concluded that the degree of impairment appears to be independent of the locus of the damage, but does vary significantly with the size of the lesion. In other words, rigidity seems to be a general rather than a specific symptom of brain-injured organisms.

In brief, experimental and clinical evidence points to rigidity as a behavior trait caused by various organismic conditions. Outstanding among these are abnormality and immaturity. This study is a preliminary investigation of the status of rigidity, as found under various conditions, and specifically poses the following question: *Is rigidity a uniform trait, or are there, rather, different kinds of rigidity, varying in quality with the particular organismic conditions?*

The field of mental deficiency presents certain advantages for a solution of this problem which are not to be found in other areas. The particular advantage of studying rigidity in mentally deficient children lies in the fact that groups of individuals can be examined that are comparable in MA and in IQ, but different in neuropsychological makeup. Such neuropsychological differences between a brain-injured (exogenous) and a familial (endogenous) type of mental deficiency have been established clinically and experimentally. The evidence for this typological distinction has been presented previously in a number of experimental studies (13, 16).

If the assumption were correct that rigidity is unitary in nature we should expect no qualitative differences of stereotypy between the endogenous and the brain-injured type of mental deficiency. Though such an hypothesis is not contradictory in itself, it is not consistent with the observed basic differences of behavior between the two types. The alternative—and more likely—hypothesis states that qualitative differences of rigidity do exist between the types. Among mental traits which characterize the two types and which probably are related to distinct forms of stereotypy are the following: The non-brain-injured or endogenous type seems to be described best as an integrated organism whose deficiency lies in a lack of differentiation of mental functions. The brain-injured type characteristically lacks integration and coherence. To illustrate: mentally deficient children of the endogenous type frequently make errors on a visuo-motor test employing the copying of mosaic patterns, but the patterns, though vague and oversimplified, usually retain the integrated nature of wholes. Brain-injured children, on the other hand, frequently do not attain unity of pattern; they are particularly confused on the fundamental relation of figure to ground. The conceptual thinking of endogenous children is rather poor and unimaginative; their concepts are either too generalized and vague, or too specific and narrow. But their trends of thought rarely show the erraticism and the abnormal fluidity of association one finds in brain-injured children.

Our hypothesis, then, relates one form of perseveration to the specific deficiency of the non-brain-injured, mentally retarded child, to his lack of differentiation, to the all too vague and all too global organization. Rigidity of the brain-injured child is assumed to be connected with the symptoms of disintegration, the incoherence of behavior, and the like.

Four experiments have been conducted to test this hypothesis. Two groups of children participated; the responses of the children whose mental retardation is ascribed to early brain injury were compared to the responses of children of the endogenous type.[2] The subjects were arranged in 18 pairs, closely matched according to MA and IQ. The mean IQ of the brain-injured groups was 68.2, ranging from 63 to 76; the mean IQ of the endogenous, non-brain-injured group was 69, ranging from 63 to 74. The mean MA's of the two groups were, respectively, 9–0, ranging from 8–1 to 11–3; and 9–1, ranging from 8–3 to 11–0.

The experiments were designed to control some of the conditions of perseveration in such a way that rigidity could be studied in terms of qualitative and quantitative differences. Reproduction of material, either visually or auditorily presented, was a common feature of all four tasks. Depending on the particular test, the child reproduced either by drawing, by speaking, or by motor action.

THE FIRST TWO EXPERIMENTS

It is well known that perseverative tendencies occur frequently if the material which a person is asked to apprehend or to remember consists of sequences of elements of the same type. These so-called digits may be numbers, words, sound patterns, light patterns, simple pictures, and the like. We meet such situations, for instance, in the training and re-education of mentally deficient individuals. In learning or relearning to read or to count, the patients have to cope with sequences of words, numbers, etc. Under these circumstances a person might perseverate by uttering one word or one number over and over again, in spite of the incongruity of the response.

The first two experiments were carried out in the light of such clinical experiences. The material consisted of rather homogeneous series of digits, viz., rhythmic tone patterns and pictures.

Experiment I: Reproduction of Tone Rhythms. Twelve rhythmic patterns were presented. By pressing a key, a tone of constant pitch and of any desired length could be produced on an oscillator. Each rhythm consisted of a number of these tones of very short and of long (one second's) duration. The subject, blindfolded, was requested to listen to each rhythm carefully, and to reproduce it immediately by pressing the key. A preliminary practice series of six patterns preceded the main series. The patterns presented are shown below (∪ short; — long).

The amount of perseveration relative to the errors has been calculated for both groups (Table 1). By perseveration is meant the replacement of a presented pattern by a pattern previously heard. For instance, a child may,

[2] As to the diagnosis of the two types, cf. (12).

Preliminary Series

(*a*) ∪ ∪	(*c*) — ∪	(*e*) — ∪ —
(*b*) — —	(*d*) ∪ —	(*f*) ∪ — ∪

Main Series

(1) ∪ ∪ — —	(5) ∪ ∪ — — —	(9) — — ∪ ∪
(2) — — ∪ ∪	(6) ∪ — — ∪	(10) — ∪ ∪ — ∪
(3) ∪ ∪ ∪ — —	(7) — — — ∪ ∪	(11) — — ∪ — ∪
(4) — ∪ ∪ —	(8) ∪ — ∪ ∪ —	(12) ∪ ∪ — ∪ —

after listening to pattern 8, reproduce in its place pattern 6.

The number of incorrect patterns not traceable to perseveration was about the same (ca. 200) in both groups. The number of errors due to perseveration, however, was much greater with the brain-injured than with the endogenous group: 31.4 per cent of the erroneous patterns produced by the brain-injured group was caused by perseveration, while this was true of only 10.9 per cent of those given by the endogenous group. This difference, according to chi-square analysis, is statistically significant at the 1-per-cent level of confidence.

We found not only quantitative, but also qualitative, differences of stereotypy. Three forms of perseveration may be distinguished: (*a*) *simple perseveration* may be defined as a single repetition of an immediately preceding pattern; (*b*) *repetitive perseveration* is the repetition of a pattern occurring more than once within the series; (*c*) *iterative (jumping), or delayed, perseveration* is a suddenly appearing repetition of a pattern which had been presented two or more trials earlier in the series.

Practically no difference in the number of simple perseverations was found between the two groups. The difference in the total amount of perseveration stemmed from the delayed and repetitive perseverations produced predominantly by brain-injured children. More than one-fourth of all errors (26.9 per cent) of the brain-injured children could be traced to either delayed or repetitive perseveration; only a small number (3.7 per cent) of these perseverations occurred with the endogenous group. These differences are statistically significant at the 1-per-cent level of confidence, according to chi-square analysis.

TABLE 1

PERSEVERATIONS OCCURRING DURING REPRODUCTION OF TONE RHYTHMS

GROUP	TOTAL NUMBER OF WRONG PATTERNS	PERSEVERATIONS (PERCENTAGE OF NUMBER OF WRONG PATTERNS)			
		TOTAL	REPETITIVE	DELAYED	SIMPLE
Endogenous	221	10.9	2.3	1.4	7.2
Brain-Injured	293	31.4	18.0	8.9	4.5
p (Chi-Square)		<.01	<.01		

Experiment II: Apprehension of Pictures. In this test, we presented cards showing simple black-on-white drawings of objects. Each card was exposed for one-fifth of a second. The child was requested to name the object as soon as he saw it. Thirty different drawings were used. They were presented in six series; each series, containing five cards, was given without interruption.[3] There was a pause of one minute between the successive series. The whole test was repeated the next day. Thus, the complete experiment consisted of 60 trials. The pictures were presented in the following order:

1. Trunk, barn, books, brick wall, logs.
2. Moon, baseball, walnut, bowl, basket.
3. Sailboat, kite, envelope, railroad tracks, teepee.

[3] The five drawings of each series were mostly similar in shape to encourage perseveration.

4. Collar and necktie, butterfly, bonfire, bug, airplane.

5. Castle, lighthouse, pump, chimney, windmill.

6. Cross, window, tree, house, church steeple.

The *results* of Experiment II were essentially the same as those of Experiment I. Brain-injured children perseverated more often than did children of the endogenous type. Again, the simple perseverations did not differentiate significantly between the two groups. The greater number of perseverations of the brain-injured children were of the repetitive and delayed types; more than one-fifth of their erroneous responses were caused by these two forms of perseverations.

TABLE 2

PERSEVERATIONS DURING RECALL OF PICTURES

GROUP	TOTAL NUMBER OF WRONG RESPONSES	PERSEVERATIONS (PERCENTAGES)			
		TOTAL	REPETITIVE	DELAYED	SIMPLE
Endogenous	220	22	2.8	6.0	13.2
Brain-Injured	326	37	11.8	9.0	16.2
p (Chi-Square)		$<.01$	$<.01$		

THE LAST TWO EXPERIMENTS

The first two experiments were designed for the purpose of eliciting perseverations sufficiently numerous for a study of qualitative and quantitative differences. In Experiments III and IV the attempt was made to analyze specific conditions leading to perseveration with each of the two types of children.

Experiment III: Dot Configurations. The particular assumption which led to this experiment was that specific factors in the mental organization of the endogenous and of the brain-injured child are responsible for perseverative behavior. If factors specific for the endogenous child personality were prohibited from coming into play, perseverations should decrease in the endogenous child but should still be noticeable in the brain-injured child. We mentioned above that diffuseness, lack in discriminative capacity, is an outstanding characteristic of the endogenous child. As is known from experiments on retroactive inhibition, even with normal individuals, a memory series consisting of similar elements to be learned in succession may bring about perseverations. Therefore, endogenous children, because of their lack of discrimination, may show perseverative behavior with a series of similar items. On the other hand, if each element of the series is distinctly different from any other, perseveration should greatly decrease or disappear in the endogenous children. In brain-injured children, however, perseveration should obtain under both circumstances if factors other than lack of discrimination were responsible for their rigid behavior.

The material in this test consisted of dot patterns which were exposed for one-tenth of a second each. The child was asked to copy each pattern immediately after exposure. A preliminary series of three patterns was employed to familiarize the child with the task.

The main test, using seven different patterns, was divided into two parts. The first part consisted of nine trials: three patterns, *similar* in general shape, were presented three times. As Figure 1 shows, the patterns of this series were of triangular shape. This series will be called the similarity series (*Sim*). The second series will be termed the non-similarity series (*Non-Sim*); it consisted of four patterns bearing similarity neither to the foregoing forms nor to each other. Each of these patterns was presented only once.

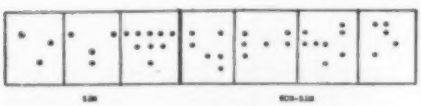

FIG. 1. PATTERNS EMPLOYED IN EXPERIMENT III

The experiment, then, should permit us to distinguish between perseverations arising from the similarity of successive patterns and perseverations not caused by this similarity. As evidence of perseveration, we considered any drawing which was exactly or nearly the same as one of the forms presented earlier in the experiment.

The results are summarized in Table 3. The total number of errors was approximately equal in both groups. This probably indicates that the test did not particularly favor one group over the other. We distinguished two kinds of perseverations: One kind occurred within the similarity series, when, for instance, a child replaced the triangular pattern of trial 4 by the triangular pattern of trial 3. The other kind consisted of the replacement of any of the patterns of the *Non-Sim* series by a triangular pattern of the *Sim* series.

All perseverations of the endogenous group occurred within the *Sim* series. In no case was any of the forms of the *Non-Sim* series replaced by a previously presented pattern of different shape. Our expectation that the cause of perseveration in endogenous children was mainly due to the lack of discrimination has been confirmed.

Brain-injured children, too, produced a great number of perseverations within the *Sim* series. But, in addition, a considerable number of their perseverations were of the second kind. That is, 30 per cent of all their errors consisted of the replacement of one of the last four patterns by a conspicuously different, triangular form.

Moreover, many brain-injured children repeated a pattern of the preliminary series when presented with a quite different form of the *Sim* series. No such perseverations occurred with endogenous children.

TABLE 3

PERSEVERATIONS DURING REPRODUCTION OF DOT-PATTERNS

GROUP	TOTAL NUMBER OF WRONG RESPONSES		PERSEVERATIONS (PERCENTAGES)			
	SIM	NON-SIM	TOTAL	SIM	SIM–NON-SIM	PRELIM.—SIM
Endogenous	34	35	44.1	44.1	0	0
Brain-Injured	41	27	69.1	39.1	30.0	14.6
p (Chi-Square)			$<.01$	....	$<.01$	$<.01$

Lack of discrimination could not be the cause of this kind of perseveration. This conclusion is also in agreement with the fact that, with the *Sim* series, where discrimination is most difficult, less perseverations were made by brain-injured than by endogenous children. It is reasonable to assume that perseverations of this sort are related to the specific syndrome of brain-injury.

Experiment IV: Reproduction of Words. In Experiment III we tried to demonstrate that conditions which elicit perseveration are not identical for children with and without brain damage. This analysis was continued in Experiment IV. There, we again attempted to control certain conditions in such a manner that perseverations specific for brain-injured children could be separated from those occurring in the endogenous group. The experiment was based on the following assumption: Only relatively discrete elements are expected to perseverate independently; because of the cohesion of a group of elements, an element embedded in a larger whole will never, or rarely, perseverate.

We set up conditions under which endogenous children, because of their global trends, were expected to experience elements as parts of larger units; we supposed, further, that these elements might still be isolated in brain-injured children with dissociative tendencies. These conditions should, then, prohibit perseverations of these elements in the endogenous group, but not in the brain-injured group.

Series of words familiar to the child were orally presented. The subject was asked to repeat each word series immediately after presentation. The entire experiment consisted of four parts with two series each. The first series of each part contained four words; the series was presented and produced four times in succession. The second series contained six words; this series was presented and reproduced only once.

Part I*a*: basket
chicken
cabbage
candle

Part I*b*: bank
children
blanket
candy
food
package

Part II*a*: penny
breakfast
dollar
evening

Part II*b*: donkey
table
everything
star
careless
company

Part III*a*: grocery
hungry
carpet
rubber

Part III*b*: city
wedding
greasy
hundred
market
ribbon

Part IV*a*: orange
station
kitty
winter

Part IV*b*: twenty
sentence
onion
ticket
tree
school

As is readily observed, the four-word series and the six-word series are of psychologically different nature. Since the four-word series was short and was several times presented and reproduced, the last reproduction was usually correct and the words were spoken in rather rapid succession. This made for

great cohesion between the four words; normally, they formed a closely knit unit. This was not the case with the six-word series. Since the series was rather long, and was presented and reproduced only once, little cohesion could be formed between the six words. The four-word group may therefore be termed a "unit"; the six-word group, an "aggregate."

Therefore, the condition for the arousal of perseverations was not much different from that of the first and the second experiment: the difficulty of recall of the discrete elements led to mutual interference.[4] This condition obviously was identical for endogenous and brain-injured children; thus, one would expect the two groups not to differ in the amount of perseverations within the

TABLE 4

PERSEVERATIONS DURING RECALL OF WORD SERIES

GROUP	TOTAL NUMBER OF WRONG RESPONSES (INCL. OMISSIONS)	PERSEVERATIONS (PERCENTAGES)		
		TOTAL	AGGR.–AGGR.	UNITS–AGGR.
Endogenous	236	5.8	2.9	2.9
Brain-Injured	260	13.0	3.8	9.2
p (Chi-Square)		$<.01$	...	$<.01$

Accordingly, two kinds of perseveration could be distinguished. One kind occurred within each "aggregate," and between the "aggregates"; the other kind occurred as a repetition of an element of a "unit" during reproduction of an "aggregate."

As in the previous three experiments, the brain-injured group produced a significantly greater total number of perseverations than did the endogenous group. Perseverations that occurred within the "aggregates" only did not differentiate significantly between the two groups. The other kind of perseveration—repetition of words of the "units" during reproduction of the aggregates—was significantly more frequent in the brain-injured group.

These results were in complete agreement with our general hypothesis if we accept the following interpretation:

1. The six-word "aggregates" were composed of loosely connected and rather homogeneous digit material. "aggregates." This expectation was borne out by the results.

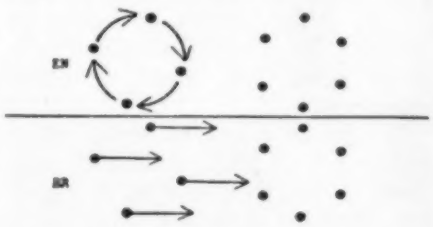

FIG. 2. DIAGRAM SYMBOLIZING RELATION BETWEEN FOUR-WORD AND SIX-WORD GROUPS IN ENDOGENOUS (EN) AND IN BRAIN-INJURED (BR) CHILDREN

In endogenous children, the four elements form a "closed" unit; in brain-injured children, discrete elements are parts of an "open" formation.

2. The four words of each series presented a closely knit group for those

[4] It is known from general psychology that mutual interference is greatest with "homogeneous" material, viz., material consisting of elements similar in kind (Koffka, 7, pp. 481 ff.).

—mainly endogenous—children who tended to perceive and reproduce in wholes, however vague these wholes may have been. We expected words of these units not to break loose and, therefore, not to interfere with the six-word series. On the other hand, we may assume that brain-injured children with dissociative trends were inclined to keep the words of the four-word groups relatively separate; therefore, they were expected to show perseveration of such words during reproduction of the six-word series (vid. diagram, Figure 2). Again, these expectations were confirmed by the results.

We should like to offer an explanation for the fact that brain-injured children, during reproduction of the "aggregates," repeated words of the units more often (9.2 per cent) than words of the aggregates (3.8 per cent). We know from clinical experience that frequency of reproduction may strengthen the perseverative tendencies in a brain-injured individual. Words of the four-word "units" were repeated several times, words of the six-word group only once. The frequent repetition of words of the "units" is a probable cause of a strong perseverative urge in brain-injured children, whereas in non-brain-injured individuals it has the opposite effect of strengthening the cohesion.

SUMMARY AND CONCLUSIONS

Four experiments were conducted for the purpose of analyzing the rigidity of brain-injured and non-brain-injured children of comparable levels of mental retardation. In all experiments the task was to reproduce visually or auditorily presented material: tone rhythms, pictures, dot-patterns, words. Quantitative and qualitative differences of rigidity, as measured in terms of perseveration, were found between the two groups.

The brain-injured children, in all experiments, produced significantly more perseverations than did the endogenous children. The rigidity of the brain-injured and that of the endogenous children appear to differ not only in amount but also in kind. The outcome of the first two experiments suggests that "delayed" and "repetitive" forms of perseveration are typical for brain-injured children.[5] The third and fourth experiments attempted to control and vary conditions responsible for certain forms of perseveration. The method permitted the separation of two kinds of rigidity related to two basically different mental structures.

An *abnormal* form of rigidity that is related to brain-injury seems to be distinctly different from a *subnormal* rigidity encountered in the endogenous type of feeblemindedness. Our previous studies have indicated that subnormality can be best described in terms of *de-differentiation;* the abnormality of brain-injured organisms, in term of *disintegration*. Inflexibility of response in the case of subnormal individuals appears to be related predominantly to "global" behavior, i.e., to perception or action organized as undifferentiated wholes. For these individuals, situations which objectively are not sharply set apart from one another may easily fuse, resulting in stereotyped responses. Lack of variability of behavior of brain-injured children seems to be caused, to a great extent, by other factors. Analysis of errors made by these children on previous, particularly sensori-motor, tests could rarely be traced back to "global" trends; much more often they were due to the opposite trend of dissecting wholes into unrelated parts.

Accordingly, repetitive and delayed forms of perseveration, found mainly in the brain-injured group, are probably

[5] Vid. also Cotton (3).

the result of isolation of certain elements of a series. These isolated elements may become self-contained and detached from the continuity of the series to such an extent that they either may be repeated over and over again or "jump" suddenly into the fore in spite of the incongruity of such behavior. The results of the dot-pattern experiment confirm the conclusion that this form of rigidity is caused by abnormal dissociation: here, dissociated responses became preponderant to such a degree that they were repeated irrespective of the patterns presented in later trials. Finally, the fourth experiment demonstrated directly the influence of isolation on abnormal perseveration. In endogenous children units of elements (viz., words) could be formed with boundaries strong enough to prohibit the perseveration of single elements. In brain-injured children, the elements remained isolated or dissociated to such a degree that they were capable of interfering with the reproduction of various other word material.

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ON THE DYNAMIC STRUCTURE OF CONCERT-PROGRAMS

BY GEORGE KINGSLEY ZIPF
Harvard University

THE present article relates to the organization of symphony programs. More specifically, it refers to the number of different composers and pieces, and the frequency with which the same were played, by the Boston Symphony Orchestra under the direction of Dr. Serge Koussevitsky during its Saturday Evening Concerts, beginning in October, 1924, with the forty-fourth season, and ending in April, 1934, at the close of the fifty-third season—ten seasons in all (or *240 concerts*).

The impetus to this research was twofold. In the *first* place, one often hears the criticism of almost any director of symphony concerts that he plays too many or too few of the "classics," or too many or too few of the "moderns," or that he plays this composition, or the compositions of this or that composer, too rarely or too often. Since all these subjective evaluations refer to an unconscious frame of reference of "the right amount," it seemed worth while to inspect several cases of program organization (of which the present one is the first) in order to disclose whether in actual concert-programs, and as the effect of the collective feelings of "the right amount," there is any governing rule or principle that can be stated quantitatively.

For, clearly, if any director—from the viewpoint of his patrons' tastes—did in fact tend in the long run to play "too little" or "too much" of any one piece, composer, or type of music, he would presumably be replaced by a more adroit successor. Since Dr. Koussevitsky has not been thus replaced, the presumption is that he satisfactorily approximated "the right amount." Therefore his programs can be used as the object of analysis.

Yet, in addition to the above impetus to this investigation, there was another. For, in the *second* place, it seemed to the present writer that this complex social question of "the right amount" or of "the right frequency" of musical items was similar in form to the complex question of "the right amount" of news items in a newspaper (1) or "the right frequency" of repetition of words in a literary composition, as has already been studied quantitatively (3). In fact, so similar are all these questions that relate to "the right amount" of repetition, on the one hand, and "the right amount" of diversification or variegation, on the other (2), that a similar answer was anticipated—as turned out to be the case.

METHODOLOGICAL PROCEDURE

The methodological procedure of studying the principle of program-organization was quite simple and, in substance, involved the following steps: First, the bound volumes of the specific programs of the ten years in question were procured from the Harvard College Library. Then Miss Rulan Chao, a graduate student of music at Radcliffe College and at the time a student of mine, volunteered to undertake, as a part of her course work, the tabulation both of the composers and of the compositions that were played during the

240 concerts. (Each successive concert was numbered in the analysis from 1 through 240, and the serial number of the concert at which each composition and each composer were played was noted.)

As a result of this tabulation it was possible to know (*a*) the *number* of different pieces played and the *frequency* of their performance, (*b*) the *number* of different composers played and the *frequency* of their performance, and (*c*) the *length of intervals* (i.e., the number of concerts that intervened) between the repetition of performances of the same composer (or of the same piece—to be ignored in this article because of the smallness of the sample).

QUANTITATIVE RESULTS

The results of the above investigation when studied statistically are quite instructive:

The Number-Frequency Relationship of Pieces Played. During the 240 concerts, 541 different pieces were played a total of 915 times. Hence approximately 60 per cent of the pieces played were different pieces, while approximately 40 per cent of them were repetitions. More specifically, 362 pieces were played once; 88, twice; 40, three times; 22, four times; 16, five times; 7, six times; 2, seven times; 3 eight times; and 1, nine times (hence this last most frequent piece was played on an average of nearly once per season).

In Figure 1 these data are presented graphically with the *f*-frequency of performance plotted logarithmically on the ordinate and the *n*-number of different pieces of like *f*-frequency plotted logarithmically on the abscissa. Thus, for example, the top point at the left at 9 on the ordinate and 1 on the abscissa represents the one piece that was played nine times; the lowest point at the right at 1 on the ordinate and at 362 on the abscissa represents the 362 different pieces that were played only once.

Even a superficial inspection of these nine points reveals that a few pieces were played often and an increasing number were played with a diminishing frequency.

Yet, viewed more closely, the nine points fall approximately on a straight line, while the lowest five points (i.e., from $f = 1$ through $f = 5$) fall almost exactly on a straight line. For reasons presently to be made clear we shall first consider these lowest five points.

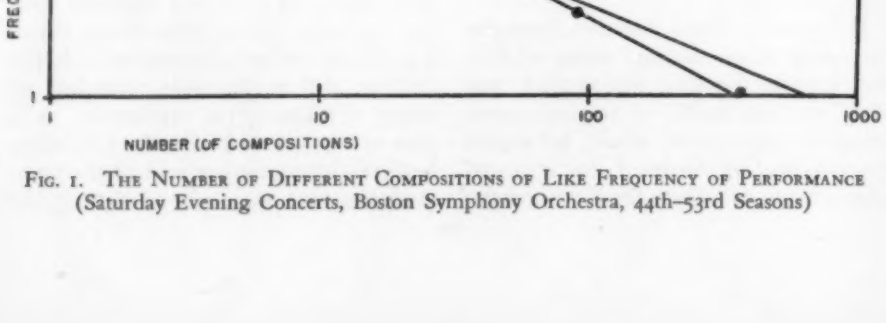

FIG. 1. THE NUMBER OF DIFFERENT COMPOSITIONS OF LIKE FREQUENCY OF PERFORMANCE (Saturday Evening Concerts, Boston Symphony Orchestra, 44th–53rd Seasons)

Now these lowest five points that fall almost exactly on a straight line represent 528—or 97.6 per cent—of a total of 541 *different* compositions played. Moreover, if we include their repeat-performances they represent an aggregate of 826 performances, including repetitions—or 90.3 per cent—of the grand total of 915 compositions played during the ten-year period.

Hence these five lowest points represent the overwhelming majority of different compositions played as well as the overwhelming majority of all pieces played. We mention this matter in order to make it clear that the upper four points (from $f = 6$ through $f = 9$) are probably out of line with the lower five points, and not the reverse.

If by the method of least squares we fit the "best straight line" to these lowest five points we obtain the shorter line of Figure 1 which has a negative slope of $.5072 \pm .0155$, and may be described by the equation:

$$n.f.^{1.9716} = 355.06$$

This equation is interesting because its exponent, *1.9716*, so closely approximates 2, or "the inverse square" which, in a very superficial theoretical discussion at the end of this paper, may be found to represent an orderly balance, or equilibrium, between the "right amount" of repetition and the "right amount" of diversification.[1]

If we now calculate by least squares the "best straight line" of all nine points we obtain the longer line of gentler slope on Figure 1, whose negative slope is $.3677 \pm .0693$ and which may be described by the equation:

$$n.f.^{2.7196} = 663.6$$

[1] The more detailed theoretical reasons for expecting the "inverse square" relationship are too extensive to be included in this article, but will be found *in extenso* in the author's book, *The Principle of Least Effort*, now being prepared for publication.

This second line does not fit all nine points as closely as the first line fitted the lowest five points. In other words, the overwhelming majority of compositions fitted the shorter "inverse-square" line with remarkable closeness.

In regard to the top four points it should be remarked, however, that four of the five most frequently played pieces were each played once at festivals (for Brahms, Beethoven, and Wagner, respectively). Curiously enough, if these festival pieces were deleted, the reallocated points would fit the first and shorter straight line more closely. Below, in reference to Figure 3, we shall discuss these "most frequent composers" again.

Yet, if, without any dichotomizing, we merely view the entire set of data as they stand in Figure 1, we may draw the almost incredible inference that the 240 programs over the ten years were somehow *organized as a whole* in respect to the number of different compositions played and to the frequency of their performances. *IF* we *explicitly assume* that the "inverse square" relationship represents about the "right amount" of program-diversity, then the subscribers to these concerts will have had little cause for complaint throughout the ten years about the *diversity* of their programs however much they may or may not have liked the particular items selected.

The Number-Frequency Relationship of Composers Played. The above data and distribution refer merely to the compositions played without any regard to their composers. If we were to rest our analysis here we should never know whether the 541 different compositions represented one composition from each of 541 different composers, or 541 different compositions of one composer—or whether the works were ordered according to some other scheme. Hence

an analysis of the composers played seemed to be indicated.

Now when we inspect the frequency-analysis of the different composers played during the ten-year period of measurement we find that the compositions of 163 different composers were played during the 240 concerts. Of this number, 151, or 92.6 per cent, were played *20* or *fewer* times, whereas 12, or 7.4 per cent, were played more than 20 times.[2] (We remember that to be played more than 20 times in ten years a composer must be played *on the average* at least twice in every season). Curiously enough, the compositions of these 12 composers aggregated 422, or 46.1 per cent, of the total number of pieces played; whereas the compositions of the remaining 151 were played 493 times, or 53.9 per cent of the total.

Let us restrict our attention to these 151 different composers whose various compositions were played *20 or fewer times*. If in Figure 2 we plot logarithmically on the ordinate the total *F*-frequency (from 1 through 20) with which a composer's works were played during the 240 concerts, and if we plot logarithmically on the abscissa the *N*-number of composers whose pieces were played with the same *F*-frequency, we find a close approximation to linearity, as becomes evident from an inspection of the actual data thus plotted in Figure 2.

For the sake of the record, the line drawn through these points of Figure 2 was calculated by least squares (with a negative slope of $.6114 \pm .2698$) and fits the equation:

$$N.F^{1.6356} = 86.34$$

[2] These 12 most performed composers, in order of decreasing frequency (with number of performances in brackets), were: (1) Beethoven [59], (2) Brahms [49], (3) Wagner [44], (4) R. Strauss [38], (5) Mozart [37], (6) Ravel [33], (7) J. S. Bach [32], (8) Debussy [28], (9) Stravinski [28], (10) Sibelius [27], (11) Tschaikovsky [24], (12) Rimski-Korssakov [23].

The linearity of the distribution of Figure 2 indicates that Dr. Koussevitsky *also* organizes his programs in terms of composers (as well as in terms of compositions as indicated in Figure 1). In other words, he not only strikes a "balanced" relationship in the diversity of his pieces; he likewise strikes that balanced relationship within the framework of a group of composers which in turn (Figure 2) is systematically ordered in reference to the *N*-number of different composers that were played with like *F*-frequency.

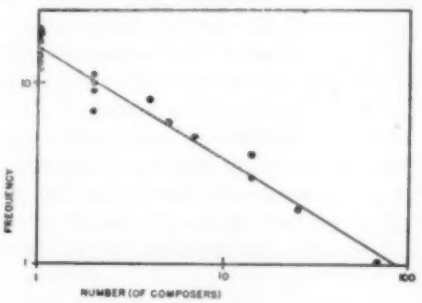

FIG. 2. THE NUMBER OF DIFFERENT COMPOSERS OF LIKE FREQUENCY OF PERFORMANCE

(Saturday Evening Concerts, Boston Symphony Orchestra, 44th–53rd Seasons)

Obviously, the fact that the slope of Figure 2 is steeper than that of Figure 1 indicates that Dr. Koussevitsky tended systematically to favor some composers at the expense of others, particularly if we *assume* that the "inverse square" represents a balanced diversity. In other words, there may have been a bias in his selection of composers.

This bias becomes all the more marked when we add the 12 most frequent composers and plot graphically in Figure 3 all the 163 different composers played during the ten-year interval when ranked in the decreasing order of the frequency with which their

compositions were played. In Figure 3 the ordinal rank (from the first through the one hundred sixty-third) with which each composer was played is plotted logarithmically on the abscissa, and the frequency with which he was played is plotted logarithmically on the ordinate. (E.g., the top point at the left is at *1* on the abscissa and at *59*

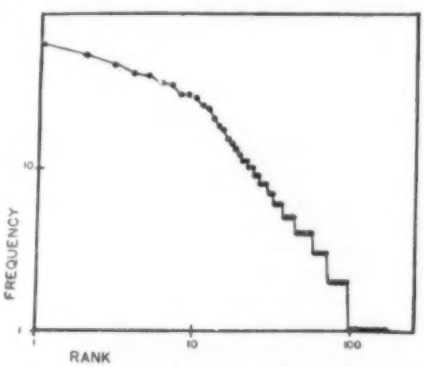

FIG. 3. THE NUMBER OF DIFFERENT COMPOSERS PLAYED, WHEN RANKED IN THE ORDER OF THE DECREASING FREQUENCY OF THEIR PERFORMANCE

(Saturday Evening Concerts, Boston Symphony Orchestra, 44th–53rd Seasons)

on the ordinate, and represents the 59 performances of the first most frequent composer, Beethoven; the next point at the right is at *2* on the abscissa and *49* on the ordinate, and represents the 49 performances of the second most frequent composer, Brahms; and so on down the line.)

Even a casual inspection of the curve of Figure 3 (whose adjacent points are joined by straight lines in order to aid the reader's eye) shows that the distribution consists of two parts: the upper and bent part, stopping at about *10* on the abscissa (i.e., where $F = 27$), and the lower and straight part beginning thereafter (i.e., where $F = 24$). This lower and straight part, save for its two first points, represents the linear data of Figure 2.[3]

The striking feature about Figure 3 is the sudden change of slope of the curve at about $r = 12$—much as if these 12 most frequent composers (which included many of the great masters of the past) were not played *proportionately* as often as those of lesser frequency which latter clearly reveal a marked rectilinear orderliness. For that matter, if we inspect the original data, it is interesting to note that, during the ten years in question, R. Strauss was played more often than Mozart; Ravel more often than Bach; Prokofieff (the thirteenth) more often than Haydn, Handel, and Schumann (the fourteenth, fifteenth, and sixteenth, respectively); that Honnegar (the seventeenth!!) surpassed Berlioz and Schubert (the nineteenth and twentieth).

One wonders whether during the ten years in question the subscribers to these concerts ever remarked about the proportion and frequency of "new" composers played. One also wonders whether during these ten years there was not a crucial re-orientation of tastes from the "old masters" to the "new," as Figure 3 seems to suggest.

In any event, the data of Figure 3 reveal that there was a schism somewhere, and that Dr. Koussevitsky was more interested in the systematic orderliness of the 151 composers that constituted the 53.9 per cent of the performances at the bottom than in the 12 composers that accounted for the 46.1 per cent of the performances at the top.

The Intervals between the Repeated Performances of Composers. The next question is the rate at which a given

[3] The fact that these data are also linear in Figure 3, though of steeper slope, is to be expected mathematically.

composer is played. Thus we may ask, for example, whether the 59 performances of Beethoven's pieces were distributed evenly over the ten years at about six per year, or whether all 59 performances occurred one right after the other, or whether the *intervals between the repetitions of performances* were arranged according to some other scheme. The same applies to the repeated performances of other composers.

In order to answer this question, we ascertained the number of concerts that intervened between the respective performances of the individual composers that were played two or more times. Thus, for example, Cesar Franck was played six times during the 240 concerts, and at the following concerts (numbered from 1 through 240): numbers 50, 99, 112, 157, 169, and 195. If we subtract each concert-number from its successor we get the respective sizes of the five *intervals* between the repetitions of the composer; thus for Cesar Franck, we obtain the *intervals* of 49, 13, 45, 12, and 26, respectively.

For every composer of *F*-frequency there will be *F—1 intervals*. *Beethoven* with his 59 performances will have 58 *intervals* in terms of intervening concerts. The 68 composers that were performed only once will have no intervals (and therefore will be ignored in the present study).

As to the frequency distribution of these *intervals* according to their size there is little than can be said *a priori*, for obviously there are any number of ways that the various composers *could* be repeated.

Nevertheless when we analyze the *number* and *sizes* of all *intervals* between the respective performances of all composers of more than one performance (ignoring the 109 zero intervals of composers that were repeated at the same concert) we find an amazing regularity, as becomes evident when we inspect the graphical data of Figure 4.

In Figure 4 are plotted logarithmically on the ordinate the *sizes of the interval* in classes of five concerts; that is, the bottom point on the ordinate represents

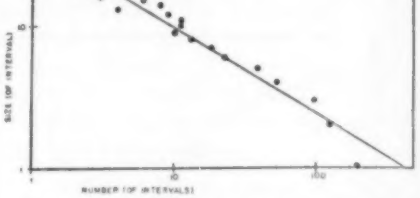

FIG. 4. THE NUMBER OF INTERVALS OF LIKE SIZE BETWEEN THE REPETITIONS OF THE PERFORMANCES OF COMPOSERS; INTERVALS IN UNITS OF FIVE INTERVENING CONCERTS

(Saturday Evening Concerts, Boston Symphony Orchestra, 44th–53rd Seasons)

intervals from 1–5 concerts, the second point represents intervals from 6–10 concerts and so on up to, and including, the topmost, or twentieth, point that represents the intervals from 96–100 concerts (there were only 14 intervals larger than 100 concerts, the largest being 167).

Hence the interval at *20* on the ordinate (i.e., the top point) signifies that over four years elapsed at some time between the repetitions of the composers in question. By the same token, the interval at *1* at the bottom on the ordinate (i.e., the bottom point) represents those instances in which a composer was performed again during the following five concerts.

On the abscissa is plotted the *number* of different instances of the *interval-classes* in question. Thus the bottom point at *193* at the right on the abscissa (and at *1* on the ordinate) represents the 193 instances in which the works

of some composer or other were performed again within the following five concerts. The top point at the left at *3* on the abscissa (and *20* on the ordinate) indicates that there were only three instances in which the works of some composer or other were repeated after an *interval* of from *96* to 100 concerts.

An inspection of Figure 4, which includes 97.8 per cent of all *intervals* (above zero), reveals two things clearly: *first*, that short *intervals* far outnumber longer *intervals;* and, secondly, that the *n*-number of intervals of like *s*-interval size is inversely proportional to a power of the *s-size*. By least-squares' calculation this power is *1.6711*, or, in equation form:[4]

$$n.s.^{1.6711} = 438$$

(the slope being—*.5984* ± *.2610*).

And so it would seem not only that Dr. Koussevitsky was orderly in the number and frequency with which he played most of his composers, but also that a high degree of orderliness attached to the rate at which the various composers were repeated. As to the rate of repetition of composers, he tended not only to repeat them sooner rather than later; but he tended to do so quite systematically.

However, in this connection let us not make the mistake of supposing that the short intervals belong exclusively to the frequently played composers, while the long intervals belong to the more rarely played ones.[5] On the contrary, 25 concerts once elapsed without a single piece by Mozart; 24 (or a full year) without a piece by Brahms; 23 without one by Bach; and 14 without a single piece by Beethoven.

Indeed, in order to stress the fact that frequently played composers *can* have long intervals, while rarely played composers *can* have short ones, we present in Table 1 the number of intervals of like size (in classes of 5 intervening concerts) for *A*, J. S. Bach, Beethoven, Brahms, and Mozart combined; *B*, Ravel, R. Strauss, and Wagner combined; *C*, all composers played 20 times; *D*, all played 10 times; and *E*, all played 4 times. (Asterisks mark the approximate average interval to be expected for the 240 concerts.)

An inspection of the data of Table 1, which are typical of all other data, shows that in all cases there are more intervals whose sizes are smaller than the average class-interval than the reverse (the mode being in each case to the left of the average, which is marked by an asterisk).

And that, in turn, implies in simple language, *just as the reader knew from his own experience beforehand*, that in virtually all concerts (and radio programs and the like) there tends to be a "run" on one composer and then on another, as vogues come and go. This "vogue for composers" has a counterpart in a "vogue for compositions" in these 240 concerts, as becomes clear from an inspection of the original data; but because of the comparatively low rate of repetition of the actual compositions and in view of our limited space, we shall not here further broach the question of the intervals between the repetition of compositions, which we shall simply assume is comparable in linearity to Figure 4.

THEORETICAL DISCUSSION

At this point the question arises as to the theoretical rationalization of our graphs. Although a more detailed dis-

[4] In the present writing we merely point out that the exponent of the equation of the number-frequency relationship of composers (Figure 2) was *1.6356*, which is remarkably close to the above exponent of *1.6711* (of Figure 4).

[5] There is a negative correlation between the frequency of a composer and the mode of the intervals between his repetitions; cf. Table 1.

TABLE 1

THE DISTRIBUTION OF INTERVALS BETWEEN THE REPETITIONS OF CERTAIN COMPOSERS PLAYED BY THE BOSTON SYMPHONY ORCHESTRA SATURDAY EVENINGS FROM THE FORTY-FOURTH THROUGH THE FIFTY-THIRD CONCERT-SEASONS (ZERO-INTERVALS EXCLUDED)

SIZE OF INTERVALS	NUMBER OF INTERVALS																				
	1–5	6–10	11–15	16–20	21–25	26–30	31–35	36–40	41–45	46–50	51–55	56–60	61–65	66–70	71–75	76–80	81–85	86–90	91–95	95–100	OVER 100
A: Bach, Beethoven, Brahms, Mozart	71	• 36	21	7	3																
B: Ravel, R. Strauss, Wagner	40	• 29	15	4	1	1															
C: All composers played 20 times	5	5	• 3	2		1	2														
D: All composers played 10 times	2	1	2	5	• 3		2	1			1	2	1	1							
E: All composers played 4 times	2		4	6	4	3		1	3	3	4	•	1	3	1		3	1	1		1

• Marks the approximate average interval of the class.

cussion of this complicated point will be found in a later publication (*The Principle of Least Effort*), a few aspects of the problem will be discussed now.

Let us consider first the data of Figure 1, which refer to the number of different compositions that were played with like frequency during the ten-year interval where we noted a very high degree of orderliness in the closeness of their approximation to the inverse square. Since the entities measured were *compositions,* or pieces, and not composers or styles of musical composition, we may conclude that these programs were planned primarily in terms of different compositions, and not in terms of different composers, or styles. In other words, Dr. Koussevitsky apparently thinks *primarily* in terms of pieces and not of composers.

Yet in addition to this first consideration there is a second one, as becomes clear when we remember that there was *also* a rectilinear orderliness in the data of Figure 2, which refer to the number of different *composers* that were played with like frequency during the ten-year interval. The linearity of this second set of data suggests that, *in addition* to thinking in terms of pieces, Dr. Koussevitsky *also* thinks in terms of composers (or possibly of their styles). Thus, for example, in planning his programs he thinks not only primarily of Beethoven's Fifth Symphony or of Brahms' Second; he also thinks secondarily of the total amount of Beethoven and Brahms that he has been presenting.

Besides the above two points, there is a third point that is suggested by the broken distribution of Figure 3, which we explained as representing some sort of a cleavage between (1) the dozen most frequently played composers whose pieces constituted about one-half of all concert material and (2) the 151 other composers whose less frequently played pieces constituted the other one-half of the concert material. Since a broken distribution of this type could be explained statistically on the grounds that the data indicate some sort of a mingling of two populations, it might well be that during the ten years in question the audiences consisted of two distinct groups—the classics lovers and the lovers of newer music—the tastes of both of whom had somehow to be satisfied. Although this suggested explanation might appeal to many of the actual subscribers to the concerts in question, other explanations are conceivable. Indeed, the data of Figure 3 seem rather to be an invitation to further inquiry than to a dogmatic statement.

In the fourth place—and theoretically the most important of all—it is evident from the distribution of intervals in Figure 4 (in which the factor of time is introduced) that Koussevitsky's decisions as to the *styles* selected are dynamic and change from year to year in response to the conflicting aesthetic tastes of his audience.

For, surely, back in 1924 Koussevitsky did not sit down with a slide rule and calculate his pieces and composers for the next ten years (far from it—the idea would be an abomination to him even as would be this present article if he ever saw it).

Instead he must have responded unconsciously, so to speak, to the length, amount, and intensity of his audience's applause, as well as to the remarks of musical critics and to whatever in the musical world corresponds to the "Letters to the Editor." A loud and prolonged applause might well be construed as a sign for an early repetition; on the other hand, the familiar Boston "upward nose-dive" might suggest the feasibility of somewhat longer delay before the next repetition. Hence it is

not surprising that short repetitions far outnumber longer ones, and that many composers never had a second trial at all. Although this statement may imply a certain truckling to the box-office and donors, the simple fact remains that box-offices and donors pay salaries! ! ! !

And so the intervals between repetitions represent the director's continual assessment of his audience's comparative likes and dislikes. They reflect his memory of their past likes and dislikes as projected into the future. But although the organizing of "balanced" concert-programs, like the editing of a "balanced" newspaper, is a "day-to-day" affair, nevertheless the cumulative effect of the adoption of new pieces and of the repetitions of the old is bound to affect the diversity and frequency of the pieces and composers that are played over a long period of time. Hence the nature of Figure 4 determines the nature of Figure 2, as becomes clear upon reflection.

For once the problem arises of repeating the performances of musical items of fixed length in concerts of an approximately fixed duration during a fixed period of years, then the diversity of items played will inevitably be affected. Thus, for example, if the audience is so pleased with a given item (whether a composition or a composer) that it wishes to hear it played at every ensuing concert, then, because of the givens of the situation, fewer other items can be included or repeated.

In short, *to increase the frequency of performance of a given item is to decrease either the diversity or the frequency (or both) of performances of other items: repetition vs. diversity.* The audience's applause therefore not only procures something for the audience but also deprives the audience of something else.

However, in speaking of audiences, let us not forget that virtually no audience is homogeneous in its tastes. For in music, too, "one man's meat may be another man's poison." An audience consists of many different positive and negative tastes. Part of the program director's job is to satisfy enough tastes and to outrage so few tastes that the concerts will continue to be attended. In practice, in any series of concerts, everyone is annoyed by some pieces and pleased by others, with old subscribers leaving and new ones entering—yet with a change in directors only when the annoyance surpasses the pleasure.

But not only is an audience not homogeneous; the individual concert-goer himself is also not homogeneous, or self-consistent, in his tastes even from day to day. Indeed, as the individual concert-goer steps into the concert hall for a concert at which *at most* only a half dozen pieces *can* be played, he is in a sense a bundle of contrarieties, since he would simultaneously like to hear both the repeated performances of many familiar pieces and also the first performance of many new ones.

By the same token, the vast majority of persons in the audience would as a group also like to hear both the repeated performance of familiar pieces and also the first performance of new ones: *repetition vs. diversity.*

Therefore the director must strike a balance, or compromise, between what we shall call the opposing "forces" of repetition and diversification. And he must strike this balance or compromise in terms of those compositions whose repetition or first adoption into the program will elicit the applause of his particular audience (i.e., he must select those pieces for repetition or adoption that most suit the audience's particular tastes).

Now *if* the opposing "forces" of repetition and diversification are of equal

magnitude, and *if* the pieces are of approximately the same size, then over a given series of concerts the *repetition of pieces* will counterbalance the *diversity of pieces,* since only one piece can be played at a time. This situation, when cast in the terms of elapsing time, suggests the relationship of an equilateral hyperbola in which the total number of different pieces played—when ranked, *r,* in the order of decreasing *f*-frequency (as in Figure 3)—will follow the equation:

$$r \times f = C$$

Furthermore, this equation, when translated into the terms of the *n*-number of different pieces of like *f*-frequency (as in Figure 1), will yield the approximate equation of the "inverse square,"

$$n.f.^2 = C,$$

which we have observed to be the case in the compositions of Dr. Koussevitsky's first ten seasons of Saturday night concerts.

So much then for the outline (the barest outline) of the theoretical explanation for the present.

Yet in this connection let us not overlook certain implications of our theory. In the *first* place, the same argument will apply to band concerts, dance orchestras, radio programs, and the like, provided that there is a continuity of audience. In the *second* place, dynamically considered, the interval-frequency relationship of Figure 4 is primary to the other relationships; in other words, because the sizes and numbers of intervals between repetitions are balanced rectilinearly on doubly logarithmic paper, it follows automatically that the number and diversity of the items played will concomitantly be rectilinear on doubly logarithmic paper because of the nature of the problem. In the *third* place, the particular items that are selected for performance and their frequency of performance are *completely relative* to the particular tastes of the audience; hence, though we may find the same statistical distributions in various kinds of concerts in this country and other countries (such as China), it does not follow at all that the same compositions or composers are represented by those distributions. It just happens that during 1924–1933 Boston liked a certain list of composers and pieces (or Dr. Koussevitsky thought they would, and apparently guessed the situation pretty closely since he was not discharged from his post). In the *fourth* place, the director must respond to the tastes of his audience, if the above equations are to be found.

Although our findings about music may seem a little unusual because of their quantitative nature, nevertheless comparable distributions have been noted on the diversity and repetition of words (3), in the size and number of news items in newspapers (1), and in speech in general (2).

For that matter, when the veterans return from the wars and sit down to recount their adventures to a fairly constant audience, they too might profitably imitate the excellent example set by Dr. Serge Koussevitsky in counterbalancing the "force" of repetition with the "force" of diversification, as they repeat their anecdotes again and again and yet again.

SUMMARY

The 240 Saturday Evening Concerts of the Boston Symphony Orchestra from the forty-fourth through the fifty-third season (October, 1924, through April, 1934), upon statistical analysis, yielded the following observations:

1. The *n*-number of different pieces of like *f*-frequency of performance was approximately inversely proportionate to the square of the *f*-frequency.

2. The *N*-number of different composers of like *F*-frequency of performance (where *F* was 20 or less) was inversely proportionate to a power of their *F*-frequency.

3. The rank-frequency distribution of composers revealed a schism between the ten most frequently performed composers and all others.

4. The number of intervals of like size (in terms of the number of intervening concerts) between the repeat-performances of composers was inversely proportionate to a power of their size.

In addition a theoretical explanation was tendered.

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A MEASUREMENT OF MOTHER-CHILD INTERACTION *

BY BARBARA MERRILL

Iowa Child Welfare Research Station

THE child's relationship with his parents is a significant factor in the development of his personality. During his earliest years the parents constitute the chief social influence which the child experiences. The techniques which the parents employ in their treatment of the child, *i.e.*, the incentives they offer, the frustrations they impose, their methods of control, together with the character of their general attitudes toward him, serve as formative forces on the child's behavior. Later, the determining nature of their interaction with the child is supplemented by forces from other parts of the environment. Nevertheless, all through the dependent years, the particular quality of the parents' roles in parent-child relationships is of paramount importance in the establishment of permanent motivational and personality attributes.

In order to understand this relationship fully, it is essential that a consistent systematic conception of the processes by which parent and child interact be formulated. The psychoanalysts have developed such a system of concepts in their treatment of various parental attitudes as they affect both child and adult personality. Stimulus-response learning theory offers another line of approach. This theory would view the parent as the social person who provides rewards, punishments, incentives, frustrations, and methods of directing the instrumental activity of the child. Consequently, the child learns certain ways of reacting to his parents, and by a process of stimulus generalization these ways are incorporated into his relationships with other individuals and so form the nucleus of his general personality.

If the child's behavior is to be studied experimentally in this theoretical framework, there are two variables to be measured: the behavior of the parent in its stimulus aspects, and the child's behavior as a response to parental stimulation.

Various techniques have been used to measure the stimulus properties of parental behavior: (1) rating scales (foremost among these are those developed by Champney [2, 3]), (2) questionnaires, (3) interviews, and (4) case histories. All these, however, offer an inadequate frame of reference for absolute measurement, thus preventing reliable comparisons outside the group studied by any one investigator. Methods 2, 3, and 4 are subject to the additional error of erroneous and selective recall, while the rating scale method involves the distorting factor of the experimenter's presence as a variable of undetermined significance.

A fifth method, that of direct observation of the mother's behavior, is subject only to the latter difficulty. Unlike the other methods, it can provide an absolute scale of recording that is comparable from one group to another, and no recall is required. The present study represents an effort to determine its effectiveness. Four main problems have been considered. In the first place, would it be possible to categorize a

* This study was done under the direction of Dr. Robert R. Sears.

mother's behavior in terms of facilitatory and inhibitory behavior and methods of control which she exercised in her handling of the child? Secondly, could such behavior be reliably observed? Thirdly, would there be sufficiently large individual differences between mothers to permit the theoretical possibility that such stimulus differences could be related to differences in child behavior? And, fourthly, would an experimental atmosphere influence the mother's behavior?

The method by which these questions were investigated was that of direct observation of a mother and her child when they were left alone together in a playroom for two half-hour periods. A running record of the mother's behavior was made with a notational system that referred to a large variety of behavior categories. Notations were made every five seconds. Therefore, the record of a given mother's performance consisted of 360 serial notations for each of the two half-hour periods. This is the method of recording used by Sears (5) in a study of individual motility of adolescent boys left in a room alone.

All subjects were treated alike at the first half-hour play session. In order to determine the effect of increased strength of motivation toward "having one's child do well," the 30 mothers who were used as subjects were divided into two subgroups for differential treatment at the second play session. Mothers were allocated to the control or experimental subgroup by a matching process based on the frequency of occurrence, at the first session, of the numerically more important categories of behavior.

EXPERIMENTAL CONDITIONS

Subjects. The *subjects* were 30 mothers together with their respective children, all of whom attended the Preschool Laboratories of the Iowa Child Welfare Research Station. The ages of the children ranged from 3–1 to 5–6. Eighteen of the children were boys and 12 were girls. All of the mothers came from the upper socio-economic brackets; five of them were wives of psychologists or psychiatrists, and, of these, three were themselves professionally trained in psychology. These background factors suggest that the subjects were, generally speaking, more familiar with modern principles of child training than a group taken from an unselected population would be.

Procedure. The procedure was the same for all subjects at the first session. When the mother arrived she was shown first to the experimental room and there given a brief explanation of the experiment. She was told that she was to go across the street to the preschool and get her child, explaining briefly to the child that he was "going to play some games," and bring him back to the experimental room for a half-hour. The room contained (1) an adult's straight chair beside a large table with magazines on it, (2) a low square tank filled with water, sand, and floating toys, (3) red and green building blocks of various sizes, (4) three small dolls representing a mother, a preschool child, and a baby, (5) assorted wood and a hammer and nails, (6) a small table with a child's tea set and two telephones on it, and (7) a small chair.

The mother was told to imagine the session as a half-hour in her own home, during which she was unoccupied by household duties and was free to be in the same room with her child. She was asked to approximate her behavior as nearly as possible to what she would do in such a situation. Any questions were answered with the statement that there were no explicit directions as to

how she should act with the child and that she was free to do as she wished. Since she had been led to believe that the research concerned the child's play behavior, these orienting statements could be considered only as the most natural under the circumstances.

The play session was observed by the experimenter from a chamber fitted with a one-way screen. The mother's behavior was recorded every five seconds in terms of the notational system described later. Timing was provided by a device that flashed a light beside the record sheet every five seconds.

After the half-hour play session was terminated and the mother had returned the child to preschool, the experimenter met her and arranged for a second appointment sometime within a week, explaining that a second session was necessary in order to obtain a reliable sample of the child's play.

The 30 mothers were divided into two matched groups, a control and an experimental group. The differentiating element consisted of the experimenter's allowing each mother of the experimental group to infer that her child's play performance in the previous session had not been an altogether satisfactory sample of his potentialities. This was designed to create an increased motivation "to have the child do well" at the second session.

At the second session, conditions were varied for the two matched subgroups of 15 mothers each. The control-group mothers were told that the procedure would be exactly the same as before. When a mother of the experimental group returned, however, the experimenter, after a few friendly remarks, inquired whether she thought her child's play had been at his best level in terms of constructiveness, imaginativeness, and maturity during the previous session. Whatever the mother's response, the experimenter agreed, but added that she considered the child capable of higher achievement and was hoping that on this second trial, when the situation was more familiar to him, his play would be more superior.

The play session itself was conducted as with the control group: If the mother seemed to show signs of real worry over her child's second performance, the experimenter was especially friendly and made encouraging remarks to the effect that, from a rough appraisal, this second trial appeared very much better than the previous one.

Each mother and child was tested for two half-hour sessions which were, with three minor exceptions, at least two days and not more than a week apart.

This experimental arrangement allowed the experimenter to investigate three problems: (1) the reliability of the first session as an index of typical behavior, (2) the effect of increased motivation to have the child perform "well," and (3) individual differences among mothers.

Behavior Categories. The categories chosen as most useful for characterizing the mother's behavior toward her child were selected partly on theoretical and partly on empirical grounds. From a theoretical standpoint it was desired to have categories which would reflect accurately the different amounts and kinds of influence the mother's behavior had on the child, *i.e.*, the stimulus properties of her behavior. From an empirical standpoint it was necessary to select categories that were clearly definable, easily recognized in the rapid flux of social interaction, and comprehensive enough *in toto* to permit the categorization of all possible behavior incidents that appeared during the play session.

The final list of categories, as it is given below, fulfilled these conditions. A running description of behavior

couched in these terms permits quantitative as well as qualitative evaluation of certain characteristics of the mother's behavior that appear to have major theoretical significance as stimuli for the child: *(a)* the degree of ***contact*** between mother and child, *(b)* the degree of ***specificity of control*** of the child's behavior by the mother, and *(c)* the degree and manner of ***facilitation and inhibition*** of the child's ongoing behavior.

TABLE 1

BEHAVIOR CATEGORIES

M = mother
C = child

a M is carrying on independent activity at the adult level—any act divorced from the experimental set-up or C, such as reading magazines, looking out the window, busying self with contents of pocketbook.

o M is a silent onlooker. M sits in adult chair, which together with the large table and its magazines are slightly apart from the toys with which C is playing; although she watches C, she is not in contact with him as explained under category *c*.

c M is in contact with C either physically, conversationally, or on a play level. "Physically" means sitting or being near C as he plays, even though she says nothing. "Conversationally" means purely social talk with C; M is apparently not trying to shape or influence C's actions. Example: "This is a nice water tank—we'll have to see if daddy can build one for you like it." "On play level" means that M is playing with C on a detached, adult level. Example: M pushes water toys around in water tank, saying, "These toys are cute—I wonder if this is supposed to be a battleship." M is in contact with C for all other categories except *a* and *o* and noncooperation when expressed in silent ignoring; therefore this category covers those cases in which no other explicit category can be referred to.

s M structurizes. She uses indirect means to stimulate or influence the child. M attempts to encourage C to think or decide for himself, usually with the aid of a few cues. M may be also trying to inspire C's imagination and creativeness. Essentially, by this form of stimulation C is oriented toward several possible courses of action, any one of which he may choose, or he is given the option of accepting one offered suggestion or not. The quality of the guidance is to C apparently unobtrusive and unimposing. With regard to the play situation, the purpose is to further and enrich play content. With reference to teaching, the aim is to have C reach his own conclusions. In respect to discipline, the basic idea is to obviate the necessity for arbitrary demands. Examples: M: "I imagine these animals would like a nice boat ride in the water." C: "But what can I use for a boat?—these little boats aren't large enough." M: "Do you see something in this room out of which you could make a boat?" C: "Oh yes, I could make one out of the wood." Or C: "Look, Mother, I just sank another enemy ship." M "That's fine. You can finish up your battle and get your ships safely back in their harbor, and then I think it will be time for us to go."

S M introduces by suggestion the possibility of play activity other than the one C is engaged in, in terms of the play units offered in the experimental room. Example: "Did you see those pretty blocks over there—and the dolls—they look as though they might need a house to live in."

t M teaches. She gives information to C for the purpose of increasing C's knowledge. Example: "This is a duck and that is a swan—swans have longer, thinner necks than do ducks." If the information is given to clarify and stimulate play possibilities, it should be marked *s*. Example: "Turtles make houses in the sand—do you suppose this turtle needs a house?" If teaching is done by structurizing, it should be marked *s*. Example: "How do swans differ from ducks?"

p M plays interactively on C's level; she plays as though she were another child. Example: "I'll be the garage man and I'll fix your car."

TABLE 1—*Continued*

ps M plays by herself while in contact with C for the purpose of interesting C in that particular play activity.

h M gives C physical help. Example: M pounds nail for C.

oh M offers help to C before giving it; in other words, she asks C's permission. Example: "Would you like me to help you carry those blocks over there?"

d M specifically directs C's actions by command or statement. She gives unequivocal impetus to a certain action; she offers C only one course of action. She tells C what to do, with the expectation that C will do that. Example: "Put that block on top of the other ones . . . then make a door right there."

i M interferes with an activity on C's part with the intent of stopping it completely. This is interpreted as a stimulus which causes a break in C's action pattern and which may conceivably be felt by C as thwarting or interrupting his course of action. Explicit directing is implied in this category. Example: "No, don't put that sand in the water . . . did you hear what mother said!"

r M restricts C's activity. She modifies his behavior by reducing intensity, speed, manner of executing, etc. Example: "Don't splash the water so high."

si M interferes by structurizing. She indicates the undesirability of a certain action and/or the consequences of the act if carried out. She suggests that it would be wiser to refrain from continuing the particular behavior and may give the reason for her judgment. The final decision is relegated to C. Example: "You know other boys and girls will want to play with those toys (floating celluloid animals) and if you mash them together like that they will be spoiled."

wc M cautions C against possible injury. Example: "You are going to hurt your thumb if you don't hold it back out of the way of the hammer."

wi M interferes with (stops) C's activity because it is involving a threat to his welfare. Example: "Don't drink that water—the cup is not clean."

w M worries over C's welfare. Example: "Oh dear, now you've gotten your suit wet and there's quite a draft in this room."

m M praises or encourages C. Example: "That's a very fine boat you've made."

n M blames, criticizes, scolds, or punishes C. Example: "You know better than to be so careless."

—q M refuses to answer C's question by being silent or evasive. Example: "I don't know—Mother is busy reading."

\+ M cooperates with a verbal request by C. The plus sign may appear in connection with an appropriate behavior category (for example, *+h*, or *+p*, indicating that the mother has agreed to help or play with the child in response to his request for such activity); or it may stand alone if none of the given categories apply.

— M ignores or refuses to comply with a verbal request by C. The minus sign is used alone or with an appropriate category in the same manner described for the plus sign.

The appropriate symbol was recorded for the particular behavior manifest whether or not it was accepted by the child or actually carried out. For example, if the mother demanded that the child do a certain thing, it was recorded *d*, no matter what the response of the child was. Similarly, if the mother attempted to stop a particular action, it was marked *i*, whether or not the child accepted the interference.

In Table 2 is given the list of categories and combined categories which have been used for the final analysis of the data. The three combined categories [*a+o*, *I*, and *n+(—m)*] require explanation. *A+o* represents the amount of time (in terms of number of five-second intervals) the mother was out of contact with the child. *I*, representing interference, is the sum of *si*, *r*, *i*, *wc*, and *wi*. All these categories represent various degrees of impingement upon the child's sphere of free activity. The category *—m* was added to *n* as a total expression of critical behavior on the part of the mother. The mother's refusal to give praise

when it was specifically asked for constituted an implied criticism of the child which probably was as effective as direct censure. All +'s were considered together, since, no matter with what category they were associated, or if they appeared alone, they represented a stimulation of the mother by the child and her positive response to that stimulation. All —'s were considered in total for the same reason, except that in this case it was a negative response of the mother.

Matching Groups. The matching of the control and experimental groups, while experimenting progressed, was based on the general picture of a mother's scores on all categories for her first session. Mothers were matched in terms of their most predominant type of behavior: those who showed a high degree of directive behavior were paired; similarly, those who had a high score in interference were matched, and so on. The categories considered for this purpose were directing, structurizing, interfering, playing with the child, and adult activity (reading magazines or silent watching).

In addition to this system of matching, the experimenter attempted to keep an equal number, in both groups, of children who played well (determined on the basis of constructiveness, maturity, and imaginativeness) and those whose play was poor (characterized by aimless wandering, purely manipulatory play, and paucity of ideas). This subjective judgment was made by the experimenter who, as a preschool teacher, is accustomed to make such evaluations; the reliability of the judgments was not determined.

TABLE 2

AVERAGE NUMBER OF FIVE-SECOND INTERVALS DEVOTED TO EACH CATEGORY OF BEHAVIOR AT THE FIRST PLAY SESSION

CATEGORY	NUMBER OF 5-SECOND INTERVALS		RELIABILITY OF DIFFERENCE; LEVEL OF CONFIDENCE
	CONTROL GROUP (N=15)	EXPERIMENTAL GROUP (N=15)	
a+o	87	91	>20%
s	20.5	11.9	>20%
S	1.6	.5	20%
t	10.9	12.9	>20%
p	48.7	34.4	>20%
h	7.2	6.9	>20%
d	14.1	11.9	>20%
I	7.2	8.1	>20%
n+(—m)	1.9	1.6	>20%
all +	7.3	9.9	>20%
all —	1.2	2.6	20%

Table 2 shows, for the control and experimental groups, the average frequency of occurrence of each of the principal behavior categories at the first session. None of the differences is statistically significant below the 20-percent level of confidence by the t-test.

Observer Reliability. The reliability of observation and recording was measured by calculating the percentage agreement between two observers re-

cording simultaneously during five half-hour periods immediately preceding the collection of the data reported here. The formula used was:

$$\frac{\text{number of agreements}}{360}.$$

The number 360 represents the total number of symbols recorded in a 30-minute period, one symbol being recorded for every five-second interval. The reliabilities of each of the last five sessions in which the two observers worked together, representing a total of 150 minutes of observation, ranged from .86 to .92, the average reliability for all five sessions being .88.

The median observer reliability for each separate category listed in Table 2 was also established. These values ranged from .83 to 1.00.

Results

The three problems to be investigated in this study were (1) the consistency of behavior from first to second session, (2) the effect of pressure applied to the mothers in the form of increased motivation for their children to perform "well," and (3) individual differences in the mothers' behavior with their children. Results bearing on these three problems will be discussed separately.

Consistency of Behavior. This variable was studied by comparing the first and second sessions of the control-group mothers. Since the conditions for the two sessions were identical, the behavior appearing in the first session should have been repeated in the second if the categories were measuring a consistent aspect of the mothers' treatment of their children. The averages by categories for the control mothers for their first and second session are presented in Table 3. When the t-test was applied to the averages, none of the differences was found to be significant.

To determine the variability of individual changes from the first to second session, the F-test (Lindquist, 4) was applied by categories to measures expressive of mean deviations for both control and experimental groups. The (+) category showed a reliable difference in variability in favor of the control group. This difference was significant at the 1-per-cent level. None of the other differences was significant for the control group. The individual records of the control group show that three cases of comparatively large change are responsible for the significant difference in the (+) category.

A direct measure of the consistency of the mothers from the first to the second session was made by rank order correlation between the frequencies of each category. These coefficients are given in the last column of Table 3. Except for the three bottom categories, which were low in absolute frequency, all values are reasonably high.

It is evident then that the experimental conditions offered the mothers an opportunity to display a consistent form of behavior. Whether this behavior could be considered a true representation of each individual mother-child relationship is another question.

Effect of Increased Motivation. It will be remembered that when the experimental mothers returned for the second session the experimenter intimated that the child was capable of playing on a higher level than had been indicated in the first session. This statement presumably increased the mother's motivation to have her child perform "well" during the second session.

A comparison of the averages for the two sessions for these mothers appears in Table 3.

Those categories which clearly showed significant differences were *d* and *I*, both significant at the 5-per-cent level. Two other categories, *S* and $n+(-m)$,

were significant at the 20-per-cent level, and each lacked 0.14 points of being significant at the 5-per-cent level.

arbitrary directing of, and interference with, the child's activity. The directing may take the form either of concrete

TABLE 3

AVERAGE FREQUENCY OF EACH CATEGORY IN FIRST AND SECOND SESSIONS, RELIABILITY OF DIFFERENCES BETWEEN THEIR MEANS AND VARIABILITIES, AND, FOR THE CONTROL GROUP, RANK ORDER CORRELATION BETWEEN SESSIONS

CATEGORY	CONTROL GROUP				
	AVERAGE FREQUENCY		RELIABILITY OF DIFFERENCE BETWEEN MEANS; LEVEL OF CONFIDENCE (T-TEST)	RELIABILITY OF DIFFERENCE IN VARIABILITY; LEVEL OF CONFIDENCE (F-TEST)	CORRELATION BETWEEN SESSIONS
	FIRST SESSION	SECOND SESSION			
a+o	87	91	>20%	>5%	.74
s	20.5	18.2	>20%	>5%	.92
S	1.6	1.3	>20%	>5%	.75
t	10.9	10.6	>20%	>5%	.86
p	48.7	41.2	>20%	>5%	.77
h	7.2	7.2	>20%	>5%	.51
d	14.1	15	>20%	>5%	.77
I	7.2	8.4	>20%	>5%	.74
n+(—m)	1.9	2.1	>20%	>5%	.33
all +	7.3	10.2	>20%	1%	.45
all —	1.2	1.2	>20%	>5%	.46
	EXPERIMENTAL GROUP				
a+o	91	71	>20%	>5%	
s	11.9	12.3	>20%	5%	
S	.5	2	20%	>5%	
t	12.9	7.6	>20%	5%	
p	34.4	23.8	>20%	>5%	
h	6.9	6.9	>20%	>5%	
d	11.9	25.5	5%	>5%	
I	8.1	14.5	5%	5%	
n+(—m)	1.6	5.2	20%	1%	
all +	9.9	7.7	>20%	>5%	
all —	2.6	3.7	>20%	5%	

These differences between the two sessions seem to indicate that a clearly structured motivation on the part of a mother to have her child realize his highest abilities, specifically when such abilities are being judged by a third person, results in a change in the quality of her interaction with the child. This change is characterized by an increased domination of the child as expressed in determination of the child's actions in ways which the mother considers will exemplify his possibilities, or of a general attitude of imposing her control because of her increased strength of motivation. The interference aspect of the relationship apparently stemmed from the mother's increased awareness of and involvement in the behavior of the child. It was probably also an

expression of uneasiness on the part of the mother. Directing and interfering are closely interrelated forms of behavior.

The increase in category *S*, which may have reflected a desire to change the child's activity to one in which his capabilities would be more easily or clearly displayed, was exemplified several times in the second session by suggestions to play with the blocks. Evidently, mothers felt that the blocks would be a medium more conducive to a demonstration of creativeness on the child's part. Another use of *S* by these mothers in the second session was in instigating more varied play with all the equipment in the room. Many of the mothers seemingly thought play with the water tank was an unfruitful occupation. This impression was founded on observation during the session and on remarks made afterward in an apologetic vein, by several of the mothers, to the effect that the child apparently wanted to do nothing but play with the water.

The increase in critical behavior [*n*+(—*m*)] represented an increase in aggressive reactions to the frustration produced by the child's failure to realize the mother's expectations. Since this motivation was strengthened by the experimenter, the frustration would be greater.

When the F-test was applied to the data to compare variability for the two groups on each category, the experimental group was significantly more variable at the 5-per-cent level on the following categories: *I*, *t*, *s*, and *(—)*; and the category *n*+(—*m*) was significantly more variable at the 1-per-cent level.

In the case of *s*, although the averages of the experimental group for the two sessions were very close, an examination of the individual records shows that very few cases remained even approximately the same; a number of mothers increased substantially in structurizing and an equal number showed a considerable decrease. These cases, of course, balanced out with respect to the averages. But this particular pattern of variation shows that some mothers definitely used structurizing as a method for inspiring superior performance on the part of the child, while others deviated from their more usual technique and substituted other methods, *i.e.*, directing and interfering in general.

The reason that directing does not show an increased variability for the experimental mothers, even though its averages show a wide difference, is that, although all but one mother increased in directing to some extent, the amount of this increase was fairly constant for the group as a whole. The same explanation holds for *S*.

With respect to *t* and (—), the F-test for variability points up the tendency shown in the averages, *i.e.*, that teaching decreased and noncooperation increased. In only three cases was there an increase in teaching. There was a decrease in noncooperation in only one instance and equality or an increase in all others.

These two statistical treatments of the data, *i.e.*, the t-test for significant differences of averages, and the F-test for significant differences in variability, show that for all categories except *a*+*o*, *p*,[1] and (—) the experimental group evidenced altered behavior.[2]

Of the experimental mothers, only two failed to respond in some detectable way to the applied pressure. In general, a mother's relationship with her child appears to be influenced and changed

[1] The category *p* lacked 0.2 points of being significantly more variable at the 5-per-cent level.

[2] The validity of the t-test is somewhat reduced for those categories in which the F-test demonstrated relatively big differences in SD.

by her motivation to have that child appear to the world in the best possible light. When this motivation is restricted to a specific situation, delimited in time, and defined in terms of performance desired, the mother tends to assume direct control of the child's actions, and to impose her own standards, rather than to interact with him in such a way as to advance his ability to think and act independently and to foster his autonomy within the limits of necessary adult guidance.

Individual Differences. Each mother presented a unique pattern of behavior as represented by the totals for the various behavior categories. A brief description of the behavior content represented by the profiles of category frequencies in three cases will give some indication of the effectiveness of this observation method as a technique for measuring the stimulus properties of a mother's behavior. The quoted remarks were taken in shorthand at the time of the experiment. Table 4 presents for these three mothers the frequency of each of the main categories in the first session, and also the change in frequency occurring at the second session.

Mother N in the experimental group displayed the most atypical behavior of the mothers tested. Even during the first session she was attempting to exhibit the abilities of her child by having him spell his name, recall phone numbers, and build certain structures with the blocks. She said, "Build something pretty for mother," and again, "Build a beautiful bridge like you do at home," and then when the construction was completed she would criticize it and remark, "But you build such lovely things at home, why don't you do it here?"

TABLE 4

FREQUENCY (NUMBER OF FIVE-SECOND INTERVALS) OF MAIN CATEGORIES AT FIRST SESSION AND CHANGE IN FREQUENCY AT SECOND SESSION FOR THREE EXPERIMENTAL-GROUP MOTHERS

CATEGORY	MOTHER N		MOTHER M		MOTHER H	
	SESSION I	CHANGE IN SESSION II	SESSION I	CHANGE IN SESSION II	SESSION I	CHANGE IN SESSION II
a+*o*	74	−29	8	−8	0	0
s	24	−13	11	0	19	+17
S	0	+2	0	0	3	+3
t	3	−1	44	−20	4	+12
p	14	−14	5	−5	4	−2
h	10	−3	8	−8	9	−9
d	38	+44	2	+13	8	+13
l	3	+10	1	+14	26	+9
n+(−*m*)	7	+12	0	0	4	+1
all +	5	+3	6	−3	4	−3
all −	4	0	0	0	1	+3

At the time of the talk prior to the second session, which was designed to increase the mother's motivation to have the child perform well, this mother assumed control of the conversation. She emphatically stated that the child had not performed at all well on the previous day, that actually he was a most precocious and unusual child, and

she suggested that the experimental toys were not sufficiently stimulating to him. Several times she expressed a desire that the experimenter could see him at home and judge his remarkable ability in those surroundings. It was evident in this case that motivation to have the child perform "well" was at a maximum.

The second session of this mother was characterized by an almost desperate attempt to force the child to display his capabilities. Table 4 shows the substantial increase in directing, interfering, and criticizing types of behavior. When her efforts in this direction were frustrated by the child's bewilderment as to what she wanted of him, she became distinctly annoyed. The *a* part of the *a*+*o* category in the second session represented her return to the adult chair and table and impatient leafing of the magazines, regardless of their content. She told the child exactly what to make with the blocks and how to make it, but at one point her annoyance was so great she said, "I don't care *what* you do—just fix something for me."

The child in this case was a four-year-old whose speech and manner were that of a miniature adult. The relationship between mother and child was evidently very close and the child showed extreme dependence on the mother. He tried sincerely to satisfy his mother's demands but such questions as "Mother, will this do?"; "Mother, how would *you* build this bridge?"; and "But don't you want me to make what *you* want?" only served to exasperate his mother since they indicated his reliance on her. The mother's demands, the child's attempt to comply with them, the mother's annoyance, and the child's bewilderment constituted a vicious circle of interaction which resulted in frustration for both mother and child.

On the basis of the recorded behavior of the mother and a subjective interpretation of the interaction between mother and child, the writer would tentatively characterize the relationship as one in which, through the process of projection, the child had become a medium for the attainment of the mother's unsatisfied desires. To the mother, then, the child possessed no personality of his own but was rather an extension of her own person and therefore logically subject to complete control. An interesting application in this case is the finding by Charlotte Buhler (1) of a high correlation between expectations which a mother has in regard to the child and the demands which she makes upon him.

In direct contrast to this case was mother M in the experimental group. This mother's behavior at both sessions was typified by a genuine interest in, and enjoyment of, her child as an individual in his own right. Her manner was quiet and poised, and she often smiled at, and laughed with, the child. In the first session her interaction with the child was based on a respect for his autonomy and at the same time on a nominal amount of guidance. Her teaching, although high in frequency, was geared to a professed and sensed desire of the child to have certain facts explained. A later discussion revealed that it was her principle to wait for the child to indicate a desire for help, guidance, etc. Her reaction to the pressure-inducing talk with the experimenter was a matter-of-fact acceptance of the possibility that the newness of the situation had restricted somewhat a sample of the child's best play in the foregoing session. Her behavior in the second session was not noticeably changed but an inter-session comparison does show an increase in directing and interfering, and a decrease in teaching. The in-

creased *d*'s were not decidedly potent in form and the increased interfering was primarily of the structurizing type. It is evident, however, that even with the most understanding of mothers the tendency is for a more pronounced motivation for the child to do "well" to operate in the direction of increased control over the child's actions.

The child in this case showed an excellent adaptation to the situation and a high level of constructive play. He apparently regarded his mother as a fine companion and enjoyed her presence. The interaction between them was characterized by equality and mutual respect. Although the child was a "real boy" in every sense of the word, he showed consideration for his mother, as exemplified in one instance by his voluntary apology for, and concern over, spilling water on her shoes.

Mother H in the experimental group was predominantly of the interfering type, although her interfering tendency was tempered by an evident realization of, and attempt to adhere to, more acceptable ways of handling the child. She was a nervous, high-strung person, and her constant contact with the child was of a compulsive nature. During both sessions she alternated between a very friendly and playful relationship, oriented toward the best interests of the child, and a domineering, annoyed reaction to his slightest deviation from her standards for his behavior. Her interferences with his activity demanded immediate and full obedience and her manner of delivering such interfering remarks was extremely harsh and of the following type: "Now stop that! . . . I said No! . . . JOHN!" (not child's name). At one point when obedience was not obtained, she slapped the child's hand. The nature of the activity to which this mother was so strenuously objecting was not intrinsically serious: for example, dumping sand in the water, accidentally spilling water on the linoleum-covered floor, putting water in the sand.

During the talk with the experimenter before the second session, the mother complained about her child's occupation with the water tank to the exclusion of all else in the room, and in the second session she used every method possible to lure him away from water play. The variety of her methods is shown in Table 4 by increased *S*, *s*, *t*, and *d*. The table does not include the category "mother plays by herself," a type of behavior in which this mother increased substantially at the second session and which seemed designed to interest the child in another activity. All these attempts were for the most part unsuccessful, and her increased interfering may have been an indication of her frustration.

In a later discussion she insistently offered detailed descriptions of his imaginative and constructive activities at home, apparently in compensation for anxiety over his "bad" behavior. Actually, the child (a three-year-old) had played quite adequately even though he had confined his interests to the water table. His reaction to his mother was characterized by indifference and ignoring, and a staunch determination to continue his own pursuits despite any and all stimuli which she presented. This attitude evidently concerned the mother greatly and, in turn, was intensified by the uncertainty and inconsistency of her treatment of him. This particular mother-child relationship was composed of a rejecting child and a mother whose basic insecurity in her dealings with the child resulted in vacillating handling of him. This latter behavior served as both a stimulus for, and a response to, the child's characteristic role in the relationship.

Summary and Conclusions

An experiment was designed to study and measure the stimulus properties of a mother's behavior toward her preschool child in a standardized play situation. The mother's behavior was recorded every five seconds in terms of 32 behavior categories, 11 of which were analyzed in the final consideration of the data: lack of contact, structurizing, structurizing a change in activity, teaching, interactive play, helping, directing, interfering, criticizing, cooperation, and noncooperation.

Thirty mothers were divided equally into a control and an experimental group, matched on predominant forms of behavior. The control-group mothers were tested in two half-hour play sessions under identical conditions. When the mothers in the experimental group returned for their second session, their motivation to have the child perform "well" was presumably increased by the experimenter's inference that the child's previous play performance had not realized his capabilities.

The control-group mothers evidenced consistent trends in behavior from first to second session. The experimental group showed a significant increase at the second session in directing, interfering, criticizing, and structurizing-a-change-in-activity types of behavior.

A study of individual differences showed a wide range of behavior patterns. Analysis of these revealed possible relationships between maternal behavior and child personality.

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ENVIRONMENT AND THE DRAW-A-MAN TEST: THE PERFORMANCE OF INDIAN CHILDREN

BY ROBERT J. HAVIGHURST, MINNA KOROL GUNTHER, AND INEZ ELLIS PRATT

Committee on Human Development, The University of Chicago

THE Goodenough Draw-a-Man Test was administered to 325 Indian children aged six through eleven in the Hopi, Zuni, Zia, Papago, Navaho, and Sioux tribes. This was one of a wide variety of psychological tests and socio-psychological methods used to throw light on the mental, social and physical development of Indian children.[1]

The Draw-a-Man Test was chosen because it seemed probable that Indian children would be under little or no experiential handicap in this test. As it turned out, the Indian children did better than white children, and the study became to some extent a study of environmental influence on children's performance on the Draw-a-Man Test.

The argument advanced by Goodenough for the value of the test is that

> in young children a close relationship is apparent between concept development as shown in drawing, and general intelligence. . . . The order of development in drawing is remarkably constant, even among children of very different social antecedents—close agreement, both as regards the method of indicating the separate items in a drawing and the order in which these items tend to appear. This is especially true as regards the human figure, probably because of its universal familiarity. (3, p. 12)

Several studies have been made of Indians with the use of the Draw-a-Man Test. Goodenough (4), in 1926, reported for 79 Indian children in the Hoopa Valley (California) Reservation School an average IQ of 85.6. These children were in Grades 1 to 4. Telford (15), in 1932, reported results on the Draw-a-Man Test for 225 North Dakota Indian children. These children ranged from kindergarten through sixth grade. The average IQ was 88.6, and there were no consistent changes in IQ with age. However, he found an increase of IQ with length of schooling for the first two years, after which there was no change. Telford was interested in the relationship between the degree of Indian blood and the test performance, and he concluded that his results show a lack of any consistent relationship between the two.

In 1933 Eells (2) reported a study of Alaskan children whom he classified as Eskimos, Aleuts, and Indians. The Indians lived in Southeastern Alaska. His results were:

	IQ	
Eskimo	89.6	(364 children)
Aleuts	93.3	(105 children)
Indians	91.6	(58 children)

Rohrer (13) reported in 1942 on a study of Osage (Oklahoma) children. The Osage Indians are well off, finan-

[1] This is one of a series of articles reporting an intensive study of the development of Indian children. Techniques used in the study were drawn from psychology, anthropology, sociology, and medical science. Representatives of these sciences worked together in planning, conducting, and interpreting the results of the study under the auspices of the Committee on Human Development of the University of Chicago and the United States Office of Indian Affairs, which supported the study financially and worked with the University of Chicago group in planning and conducting it. Articles like the present one will appear in appropriate journals to describe the various aspects of the study. These will serve to document a series of monographs designed to integrate and interpret the material of each of the tribes.

cially. English is their language. Their children go to the public schools. The Draw-a-Man Test was given to 125 (out of 331) Osage children in Grades 1 to 3 of the elementary schools of a community. Children in outlying schools were omitted. An equal number of white children in the same classrooms were tested. The mean IQ for the Indian children was 103.8; for white children, 102.9.

Dennis (1) gave the Draw-a-Man Test to 152 Hopi children aged six through ten, in 1941. The average IQ was 108.3, with a significant sex difference. Boys averaged 116.6 and girls averaged 99.5. Since many of the children tested by Dennis were tested again in the present study, our results will be of special interest in comparison with those of Dennis.

Russell (14) gave the Draw-a-Man Test and collected spontaneous drawings from 41 Zuni children. He found an average IQ of 105, with no significant sex difference.

SELECTION OF SUBJECTS AND SAMPLING PROCEDURES

Children from six Indian tribes were tested in the present study. The tribes were: Sioux, Navaho, Papago, Hopi, Zuni, and Zia. The Hopi, Zuni, and Zia Indians practically all live within their respective pueblo towns. The other tribes are much larger in population and cover wider areas of land. The Navaho, with 50,000 people, inhabiting an area of 23,570 square miles, much of which is unfit even for grazing, are spread over the widest territory. The Sioux and Papago have large reservations, with a dispersed population. The Hopi number about 3500 living in eleven villages on three mesas surrounded by the Navaho Reservation.

Where the population is dispersed, as on the Sioux, Navaho, and Papago reservations, there is usually a wide variation of living conditions, economic welfare, amount of schooling, and contact with the neighboring white culture. Consequently, two or more communities were chosen for study in these tribes in an effort to discover variations within a given tribe.

When the communities which were to be studied had been selected there remained the problem of selecting the children to be tested. The principles followed in selection of subjects were: (1) All children from six through eleven were to be tested unless the number was too large. (2) If the number was too large, a random sample of this group was to be made if possible. (3) If a random sample could not be obtained, the sampling procedure was to be described and recorded in detail.

The list of children, with their ages, was in each case taken from the school census. In some tribes the birth dates are not always definite or accurate. The recorded age of a child was checked by asking him and his parents his age. In some doubtful cases, church records were of value in establishing the age. Where the contact with white culture was precarious, birth dates were not available for all children and in a few cases the field workers had to be content with getting an estimate of the year of birth.

Information is summarized in Table 1 for each of the tribes, giving number of children in the various communities and numbers actually tested, and describing sampling procedures that were used.

The Arthur Performance Test was given to the same groups of children (5). We succeeded in giving the Arthur test to a larger proportion of our sample than the proportion from whom we secured drawings. We got drawings

TABLE 1

DATA ON SELECTION AND SAMPLING

TRIBE	HOPI		ZUNI	ZIA	NAVAHO	PAPAGO		SIOUX	
COMMUNITY	ORAIBI	FIRST MESA			SHIPROCK	TOPAWA	HICKIWAN-GUVO	KYLE	PINE RIDGE
No. of children 6 through 11	53	52 *	c. 300 †	36	c. 150 ‡	60	59 §	c. 100 ‖	c. 300 ‖
No. of children in sample	53	52	42	36	57	60	59	43	26
No. of children tested	46	32	42	32	47	48	25	30	23

* Excludes children from the Tewa-speaking pueblo of Hano, and also some 25 children of predominantly Tewa affiliation in the other pueblos.

† About half of the children attend the Indian Service day school, the others going to a Catholic or a Christian-Reformed mission school. There is said to be no great difference between those who attend a mission school and those who attend the government day school. Every sixth name was taken from the Indian Service school roll and these children, together with their siblings, made up the sample.

‡ This number is a rough estimate. There were 57 children attending day school, and they constituted the sample.

§ In this isolated area, about 40 per cent of the children have never been to school. Most of those who were tested were in school.

‖ The research supervisor, a stranger to the communities, selected the samples so as to make the blood mixture conform to the pattern of the reservation, with correct percentage of full bloods, intermediate mixed bloods, and predominantly white mixed bloods.

from more than 80 per cent of those who took the Arthur test in six of the nine groups. The range was from 42 to 100 per cent. Accordingly, we used the Arthur test data on those who took the Draw-a-Man Test to find out whether our Draw-a-Man groups were representative samples.

We have tried to determine how representative our samples were by asking two questions: (1) Is the group from whom we secured drawings representative of the larger group whom we tested with the Arthur test? (2) Is the group tested with the Arthur test representative of the entire group of children in the community?

In answer to the first question we inferred that a group from whom we secured drawings was representative of the larger group who took the Arthur test if the mean Arthur IQ's of the "Arthur" group and its subgroup were nearly the same. This proved to be the case in all of our groups. The greatest difference between the mean Arthur IQ's of the larger group and the group for which we have drawings is 2.8 points, in the case of the Oraibi (Hopi) group. This difference could easily occur as a result of chance.

In answer to the second question, we are sure in some cases that the group tested with the Arthur test is representative of the entire group of children in the community, and uncertain on this point in other cases.

We can put the groups into categories with reference to the confidence we may have that the group actually tested with the Draw-a-Man Test represents the total group of children in the community. These categories are:

A. The group tested is almost surely representative of the total group.
   Zia, Hopi (Oraibi)

B. The group tested is probably representative of the group given the Arthur test, and this group is probably representative of the whole group.
   Zuni, Sioux (Kyle), Hopi (First Mesa)

C. We cannot say that the group tested is representative of the whole group, but the group tested is probably representative of the group given the Arthur test.
   Sioux (Pine Ridge), Navaho (Shiprock), Papago (Hickiwan-GuVo)

The Papago (Topawa) group does not fit any of these categories and will be discussed later.

ADMINISTRATION AND SCORING OF THE TESTS

The test was administered by teachers in the schools which the children attended during the school year 1942–43. In most cases, the regular classroom teacher administered the test. In a small proportion of cases, one teacher gave the test to all the children in a school. A few tests were given by field research workers to children who were not in school. The Draw-a-Man Test was given as the ninth drawing in a series of drawings or paintings, of which the first eight were "free drawings." In the first eight drawings the children were free to choose size of paper, medium, and subject. The teachers were instructed to exert just as little influence as possible. The "free drawing test" was a projective test, designed for the study of personality manifestations. This free drawing experience probably had some effect on the children's Draw-a-Man performance. Although the teachers were given the instructions in Goodenough's manual, teachers as well as children appear to have been habituated to the "free drawing" conditions so that a considerable variety of drawings resulted from the Draw-a-Man assignment. A few children colored their drawings, others drew women, and others drew men in the act of performing various operations. While this

unusual freedom made scoring difficult, and caused us to throw out a few drawings in which significant details were covered by paint or were hidden behind other objects, it probably reduced the tendency of the teachers to make suggestions to the children about their drawings. Thus the drawings were obtained when teachers and children were relatively "relaxed." The Navaho (Shiprock) drawing test was given first in the series, before the "free drawings."

The tests were scored according to Goodenough's standards.[2] We scored drawings for children from six through eleven, whereas Goodenough does not recommend the test for use with subjects older than ten. Our reason for using the tests of the eleven-year-olds was to secure more cases for statistical study. The effect of including eleven-year-olds in the study is to lower the average IQ of the group.

RESULTS

The mean IQ's obtained on the Draw-a-Man Test for the various groups of children in the study are shown in Table 2. This table also shows the mean Arthur IQ's for the Draw-a-Man groups, and the mean Arthur IQ's for the larger groups who were tested with the Arthur test. This last figure is for children in the age group six to fifteen, and therefore is based upon a larger group than the number actually shown in the first column. Since there was no systematic variation of Arthur IQ with age, comparison of the mean values of the Arthur IQ for the larger group with the mean values of the Arthur IQ for the smaller group should help us to decide whether the smaller group is representative of the larger group.

Table 2 also gives the Draw-a-Man IQ's for a group of white children in a small middle-western city. This group comprised nearly all of the ten-year-olds in the town, and is a fairly good cross-section of middle-western white children. All but eight of the 66 children were ten years old, the other eight falling between eleven and eleven years and three months. For this group of white children we do not have Arthur IQ's, but we have IQ's on the Cornell-Coxe Performance Test which has some elements in common with the Arthur test.

The Indian children exceeded the norms for white children by a statistically significant amount in seven of the nine groups. They exceeded their own Arthur IQ's in seven of the nine groups. The white children scored lower on the Draw-a-Man Test than on the Cornell-Coxe. It appears that the Indian children gave a definitely superior performance to that of white children on the Draw-a-Man Test.

Mean IQ's and standard deviations are given for each tribal group and for the midwest group on the Draw-a-Man Test in Table 3.

Sex differences were manifested in several of the groups. In the Pueblo groups (Zuni, Zia, and Hopi), the boys were definitely superior to the girls at all age levels between six and eleven. The Sioux boys showed a smaller superiority over the girls, but one which is statistically significant. The Navaho group showed no difference between the sexes. The Papago Topawa group showed the girls definitely superior to

[2] While we were developing our scoring technique, we sent a set of 20 Indian children's drawings to Miss Goodenough, who helped us by asking one of her research assistants, Miss Harriet Blodgett, to score these drawings. We compared our scores with those of Miss Blodgett, and changed our methods on several points to agree with hers. Each of our drawings was scored by two persons. One of us (M. K. G.) scored all the drawings, and the others of us (I. E. P. and R. J. H.) each scored half of them. The two scores for each drawing were averaged.

TABLE 2

DRAW-A-MAN IQ COMPARED WITH ARTHUR IQ

	No. Given Draw-a-Man Test Age 6–11 *	Mean IQ Draw-a-Man, Age 6–11 *	Mean IQ Draw-a-Man, Age 6–10 *	Mean IQ Arthur, Age 6–11 *	Mean IQ Arthur, Age 6–15 *
Hopi (Oraibi)					
Boys	24	120.5	123.2	116.6	
Girls	22	99.5	102.3	107.4	
Total	46	110.5	112.7	112.2	115.0
Hopi (First Mesa)					
Boys	15	126.7	133.8	110.5	
Girls	17	108.6	108.9	111.2	
Total	32	117.1	119.6	110.9	110.7
Zuni					
Boys	18	122.1	122.1	99.6	
Girls	24	104.0	107.8	100.0	
Total	42	111.7	114.5	99.9	99.6
Zia					
Boys	17	116.4	120.1	96.6	
Girls	15	101.9	100.9	92.2	
Total	32	109.6	110.9	94.5	96.5
Navaho (Shiprock)					
Boys	20	110.1	107.0	98.6	
Girls	27	109.4	110.8	91.3	
Total	47	109.7	109.5	94.4	95.9
Papago (Topawa)					
Boys	24	101.8	102.1	93.0	
Girls	25	115.2	116.6	106.4	
Total	49	108.5	109.7	99.7	99.4
Papago (Hickiwan-GuVo)					
Boys	13	104.9	106.9	84.1	
Girls	12	102.2	103.4	90.8	
Total	25	103.6	105.2	87.5	86.9
Sioux (Pine Ridge)					
Boys	10	107.8	107.3	100.5	
Girls	13	97.6	99.2	101.6	
Total	23	102.0	102.6	101.1	102.6
Sioux (Kyle)					
Boys	14	116.9	117.8	97.7	
Girls	16	110.3	112.2	100.1	
Total	30	113.6	115.0	98.9	101.1
Midwest (Whites)				(Cornell-Coxe)	
Boys	28	98.3		116.0	
Girls	38	103.4		110.2	
Total	66	101.2		112.7	

* Inclusive.

the boys on the Draw-a-Man and the Arthur tests. The Hickiwan-GuVo group showed no statistically significant difference between the sexes, in a small sample. The middle-western white group showed a slight superiority of girls over boys on the Draw-a-Man Test, while the boys had an equal margin of superiority over the girls on the Cornell-Coxe test. Neither of these differences is statistically significant.

Dennis's results for his total group are slightly lower than ours. This is probably due to the fact that he included one school (Hotevila) which did not appear in our sample and which tested lower than the other two schools. He found a mean IQ of 108.3 for 152 children, aged six through ten. Our data gave an average of 115.6 for 66 children, aged six through ten. Dennis found the girls to average 99.5 and the boys 116.6.

TABLE 3

MEAN DRAW-A-MAN IQ'S AND SD'S FOR TRIBAL GROUPS

GROUP	HOPI	ZUNI	ZIA	NAVAHO	SIOUX	PAPAGO	MIDWEST (WHITE)
No.	78	42	32	47	53	74	66
Mean IQ	113.3	111.7	109.6	109.7	109.1	106.9	101.2
SD	17.4	20.7	17.2	16.3	16.9	20.6	19.0

Our results on Hopi children can be compared with those of Dennis (1), who tested most of these same children a year earlier with the Draw-a-Man Test. Our results are in substantial agreement with those of Dennis. He has given us his IQ's for the 64 children who were tested by us as well as by him. Omitting one case on which we disagreed with him about the age of the subject, we present a comparison of the results in Table 4.

We found the girls to average 105.6 and the boys 127.0. The difference between the sexes Dennis found to be 17 points, while we found it to be 21 points. The product-moment coefficient of correlation between our results and Dennis's was 0.52.

Dennis found the average IQ of the girls to decrease with age, while that of the boys increased with age. His differences were not quite statistically reliable. We, too, found the average IQ

TABLE 4

COMPARISON OF CHICAGO AND DENNIS RESULTS ON THE HOPI DRAW-A-MAN TEST

	ORAIBI			FIRST MESA		
	No.	MEAN IQ		No.	MEAN IQ	
		CHICAGO	DENNIS		CHICAGO	DENNIS
Boys	14	120.8	115.9	15	130.2	128.2
Girls	17	98.9	98.9	17	107.1	107.5

of the Hopi girls in this test to decrease with age, but that the boys remained practically constant.

To study further the relation between age and Draw-a-Man IQ, we divided our subjects into three age groups, 6–7, 8–9, 10–11; and calculated mean IQ's for each sex and each community. There were no consistent trends with age. We then combined both sexes and all the tribal groups into four age groups, with results as shown in Table 5.

TABLE 5

DRAW-A-MAN IQ IN RELATION TO AGE

AGE	6–7	8–9	10	11
Number	98	105	69	51
Mean IQ	112.2	108.3	110.2	105.8

Our results on Zuni children do not agree very well with those of Russell (14), who found a mean IQ of 105 with no sex difference. Our girls' scores agree fairly well with those of Russell, but our boys' scores are much higher. Since Russell's sample and our sample of boys are both small, 14 and 18 respectively, it is natural to look into the sampling procedures as a possible source of the differences. Our sampling procedure has been described. Russell tested the children in the summer school operated by the Indian Service in the farming community of Nutria, where a portion of the Zuni residents live each summer. It seems possible that some selective factors may have operated to send an atypical group of boys to summer school in this community.

To find out what relation there was between performance on the Draw-a-Man Test and performance on the Grace Arthur Performance Test, we calculated product-moment correlation coefficients for the Zuni, Hopi (Oraibi and First Mesa combined), Navaho (Shiprock), Sioux (Pine Ridge and Kyle combined) and Papago (Topawa) data. The results are shown in Table 6.

TABLE 6

CORRELATION COEFFICIENTS OF DRAW-A-MAN AND GRACE ARTHUR IQ'S

GROUP	r
Zuni	$.10 \pm .10$
Hopi	$.21 \pm .07$
Navaho (Shiprock)	$.23 \pm .09$
Sioux	$.33 \pm .08$
Papago (Topawa)	$.64 \pm .06$

These coefficients are quite low, with the exception of that for Topawa. They are much lower than we should expect among white children. For the midwest children the correlation coefficient of Cornell-Coxe IQ with Draw-a-Man IQ is $.63 \pm .05$. The correlation coefficient of Stanford-Binet IQ with Draw-a-Man is $.50 \pm .06$ for the midwest group. Goodenough found an r of .75 between Stanford-Binet and Draw-a-Man IQ's for white children.

DISCUSSION OF RESULTS

We shall organize our discussion around the following topics:

1. A critique of the Draw-a-Man Test.
2. The superiority of boys over girls in most of the Indian groups.
3. The superiority of Indian over white children.

Is the Draw-a-Man Test a useful test of intelligence for Indian children of the tribes we have studied? We cannot answer this question with finality, for we lack data on an undeniably true criterion of intelligence for these children, such as their ability to make socially and personally satisfactory re-

sponses to the problems with which they are commonly confronted in their lives. Our answer must be indirect and somewhat unsatisfactory.

Many test experts believe that the Arthur is a better test of general intelligence than the Draw-a-Man because the former consists of a variety of types of performance, and the latter of but one type. If we should assume that the Arthur Performance Test is a good test of general intelligence for these children, we would infer from our data that the Draw-a-Man test is not a good test of general intelligence for any of our groups except possibly the Papago (Topawa) group, because the coefficients of correlation between Arthur and Draw-a-Man IQ's are low for all except the Papago (Topawa) group.

Following out this assumption, Goodenough's argument for the close relationship in young children between concept development as shown in drawing and *general* intelligence might be modified to suggest that concept development is *one aspect* of general intelligence. As such, it may or may not be closely related with other aspects of general intelligence. In this case the Draw-a-Man Test might be expected to measure this one aspect of intelligence. If so, this aspect—namely, concept development—appears to be more closely related to other aspects of intelligence in white American culture than in most of the Indian cultures which we have studied.

It might be supposed that to the extent that Indian children are living in organized groups which are successfully meeting the stern conditions imposed on them by the natural environment and by their own social organization, they would be stimulated to observe, take an active interest in and deal in a systematic, organized manner with the world of man and nature, and to form unified concepts out of the flux of particulars observed through their senses. As a result, the children would develop the ability at an early age to form concepts of the detailed form and organization of the human body which are tested in the Draw-a-Man Test.

Dr. Laura Thompson has applied this hypothesis to the Hopi groups, as follows: [3]

> One sex, (the boys) is stimulated to become aware of, take an active interest in and express through the medium of ritual dramas certain aspects of the world of nature (including man, animals, kachinas, the elements, etc.) while the other sex (girls) have interests increasingly narrowed with age to house, household tasks, etc. The life which the boys lead, and especially their economic and ceremonial responsibilities, forces them to become more observant and to express themselves aesthetically more than does the life of the girls. The ceremonial complex (especially at First Mesa, where it functions practically intact) provides channels of logico-aesthetic development and expression including the song, the dance, the prayer, graphic arts, and impersonation. On the other hand, the Hopi culture offers the girls less rich mental and emotional development and less creative outlet at the aesthetic level. At Oraibi this is mainly in the form of plaque-making, in which the medium limits the design development more than is the case at First Mesa where painted pottery is the main medium of aesthetic expression.

Drawing experience is probably one of the many interrelated factors which may be operating to produce the result revealed by the test. Concerning this point it should be noted that many of the Indian tribes whose culture and social organization have remained relatively intact in the face of intrusion of white American culture use drawing and painting as a part of the routine of life. Adults in every family do some drawing and painting. The child observes this and imitates his elders in

[3] Private communication from Dr. Laura Thompson. For a full account of the lives of Hopi boys and girls, see (16).

the artistic activities which are appropriate to his sex. Furthermore, the Indian Service schools, in recent years, at least, have encouraged children to draw and paint by providing materials, allotting time, and rewarding the more successful ones with opportunities to study art in boarding schools and to sell their products.

We shall now sum up our hypothesis and then attempt to apply it in the interpretation of our results. The Draw-a-Man Test may be a useful test of one aspect of general intelligence, namely, the ability to form concepts based upon observation. The children of certain Indian tribes are especially stimulated to observe the external world accurately and to form concepts of natural objects, including the human body. Moreover, they are given plentiful opportunity and even encouraged or required to draw and paint these concepts.

How does this hypothesis account for the observed sex differences among the various tribes? The Pueblo boys and the Sioux were definitely superior to the girls. There was no difference between the sexes in the Navaho group. The girls were superior to the boys in the Papago (Topawa) group.

Superiority of the boys over the girls in the Hopi group has already been explained according to the hypothesis. Similar considerations apply in the Zuni and Zia pueblos. An anthropologist writes the following of the Zia pueblo:

> From the time a boy can handle a pencil or a brush he is encouraged to draw and to paint. His subjects are usually animals, although cowboys riding or roping are likewise frequent subjects. Parents and siblings encourage the youngsters with admiration and criticism. Girls rarely draw pictures except in school, but by the time the children start school the boys are much more advanced in their ease of handling pencils and colors and even more in their ability to put their accurate observations of animal forms and movements onto paper. The pueblo expects its boys to be able to paint animals upon the house walls at Christmas to encourage fertility, as well as to work later at painting the ceremonial masks, altars, and other ceremonial paraphernalia. Girls are expected to paint nothing but the conventionalized designs used on pottery. (9)

Some further information is given by the analysis of the free drawings of the children, which shows that the boys draw more animals and human figures than the girls, who draw more houses. Table 7 reports the relative frequencies of animal or human figures in the free drawings for the various tribes (11).

If we had only the results from the Pueblo groups, we should point out that the boys draw human and animal figures almost twice as often as girls, and we should suggest that in this fact lies part of the explanation of the boys' superiority over the girls in the Draw-a-Man Test. But the Navaho (Shiprock) boys also exceed the girls in the frequency of their human and animal drawings, yet they do not do better than the girls in the Draw-a-Man Test. And the Sioux boys and girls draw such figures equally often, yet the boys exceed the girls on the test. Consequently, the relative frequency of drawing human and animal figures under the conditions of our "free drawing" test cannot be taken as a strict correlate of performance on the Draw-a-Man Test.

Analysis of the free drawings of the Hopi children with respect to their treatment of space and their use of the medium was made by a university teacher of art. He found that

> the boys draw with increasing assurance, as though they felt secure in what they are doing, whereas the girls seem to draw with increasing hesitation and uncertainty. The difference becomes more pronounced as they grow older.—The drawings indicate a generally superior ability of boys over girls. (12)

The superiority of Papago (Topawa) girls over boys in our data is probably

due to an accident in our sampling for the Draw-a-Man Test. In Table 2 it will be seen that in the group which took this test the girls exceeded the boys on the Arthur as well as this test. Yet in the larger Topawa group (aged 6 through 15) there was no significant sex difference on the Arthur test. Hence it would seem reasonable to suppose that the smaller group (aged 6 through 11) which took the Draw-a-Man Test was not a representative sample of Topawa children. This supposition is partially verified by the fact that the four girls aged 6–11 who took the Arthur but not the Draw-a-Man Test averaged 87 on the Arthur test, while the five boys who took the Arthur but not the Draw-a-Man Test averaged 99 on the Arthur test. If these children had all taken the Draw-a-Man Test, it seems probable that the sex difference in favor of the girls on the Draw-a-Man Test would have been somewhat reduced.

To find out whether there was anything in the life of Papago children that would favor one sex or the other on a test of this sort, we asked Mrs. Ruth Evelyn Jones, the teacher on the Papago Reservation who supervised the administration of the drawing test, to describe the typical experience of Papago children with drawing. She writes that there is the usual sex difference in the objects drawn by boys and girls in their free time in school, but that one sex is not encouraged more than the other to practice drawing.[4]

The results of the testing in the other Papago (Hickiwan-GuVo) group do not throw much light on the question.[5] The number of cases was small, and a considerable proportion of the children did not attend school and were not tested. Hence we cannot be sure that we have a representative sample from this group.

Our hypothesis may be applied to the Sioux as follows: As a part of their adaptation to a hunting and fighting way of life, the Sioux developed a tradition of scouting, which required keen systematic observation of details and memory of them in interrelationship, even though their significance was not immediately known. This tradition further included the putting-together of details into constructs, such as "so-and-

TABLE 7

SEX DIFFERENCES IN RELATIVE FREQUENCIES OF ANIMAL OR HUMAN FIGURES IN FREE DRAWINGS

(Percentage frequencies of animal or human figures, ages 5–18)

GROUP	ORAIBI	FIRST MESA	TOPAWA	HICKIWAN-GUVO	ZIA	ZUNI	KYLE	PINE RIDGE	SHIPROCK
Boys	13	31	14	18	44	28	12	19	25
Girls	8	19	11	19	31	16	15	17	13

[4] For a full account of the lives of Papago boys and girls, see (8).

[5] One case of the Hickiwan-GuVo group was excluded in calculating the mean score because it deviated unusually far from the mean. This was a case of a boy aged six years and seven months who earned an IQ of 189 on the Draw-a-Man Test. On the Arthur test he was sixth in the group of 25, with an IQ of 104. His teacher, unaware of his superior standing on the Draw-a-Man Test, wrote the following about him, "J—— is interested in none of his school work except drawing, which he does very well. It is very hard to get him to study. As soon as my back is turned, he starts drawing or coloring. Often I have to take his paper and pencil away to get him to do his work." As is customary in calculating the mean of a small number of scores, the extreme score of this boy was omitted in calculating the mean of his group.

so has been in this neighborhood recently," "a person with such-and-such personal appearance must have left these marks of his presence." This was part of the male tradition, and was handed from father to son. It is still in the male tradition for the Sioux, although the fighting and hunting life has dissolved. Dr. Macgregor (10), anthropologist working in the Sioux country, reports that keen observation and memory for details are striking characteristics of present-day Sioux, and that boys are encouraged by the men to train themselves in this way. Homburger (7), in his report on child-rearing among the Sioux, says that the little boys are encouraged to play hunting and cowboy games. These would keep alive this tradition.

Thus the superiority of Sioux boys over girls on the Draw-a-Man Test may be explained as due to training in the formation of concepts based upon observation. But the Kyle group, both boys and girls, are superior to the Pine Ridge group. How is this to be explained? Assuming that the Pine Ridge tests are a representative sample of the performance of Pine Ridge children, which has not been established, the following explanation seems possible: Kyle is a rural community, while Pine Ridge is the reservation headquarters and is much more influenced by white culture. The Sioux tradition is much stronger at Kyle. Furthermore, the children in the Kyle community, both boys and girls, are actively engaged in work with animals and with the land, and they roam the countryside more freely. These facts suggest that the Kyle group might be expected to be superior generally to the Pine Ridge group on a test of concept formation based on sense observation.

The lack of sex difference in the Navaho (Shiprock) tests is explained by Dr. Dorothea Leighton as follows:[6]

> It seems quite understandable that little or no sex difference appears on the Shiprock tests which are limited to children under 12 years. There are no art expectations of Navaho children of either sex, and little graphic art experience. Both sexes see ceremonial drypaintings, see their mothers weave designs in blankets, may see their fathers make silver jewelry. Few girls would be weaving designs by the age of 11 or 12, equally few boys taking any part in silverwork. At the same time, both sexes are accustomed to learning by observation, both have experience of being sent out to herd sheep, to take them to water, to find roaming horses. All of these items would tend to make Navaho children observant of details of nature, including probably human figures, but would give neither sex any special advantage as compared with the other.

The general superiority of the Indian children over white children on the Draw-a-Man Test is the final fact to be discussed in the light of our hypothesis. According to this hypothesis, the children of Indian tribes which have kept close touch with the world of nature and with their indigenous cultures are specially stimulated to observe accurately, to organize their observations and express them aesthetically, and thus they may be expected to do well on the Draw-a-Man Test. White children, and urban white children especially, may have much less chance to form concepts from first-hand observation, but must rely more upon books and words. Following this line of thought we should expect the Indian groups which have been most adapted to the white culture to test more nearly the same as white children. The Pine Ridge group of the Sioux is closest to the white culture, and does test closest to the white norms. The Kyle group of the Sioux are less affected by the

[6] Private communication from Dr. Leighton. For a full account of the Navaho and their children, see Clyde Kluckhohn and Dorothea Leighton (9).

white culture than are the Pine Ridge group, and they score higher on the test. However, this small bit of proof cannot be relied upon heavily, for the Pine Ridge group which we tested was small and may not have been a good sample.

The study made by Rohrer (13) tends to confirm our hypothesis. In his study, Indian and white children who attended the same public school in Oklahoma and lived in much the same way came out about equal and close to the white norms. On the other hand, the earlier studies of the performance of Indian children on the Draw-a-Man Test, reported by Goodenough (4) and by Telford (15), gave these children very low IQ's, and yet they were living on reservations, cut off pretty much from participation in white culture. To understand these earlier results we should probably need to know about the morale of the children, of their families, and of their school teachers, whether the children had grown up in their own homes or in boarding school, and what the school practices were with respect to drawing and painting in those days.

SUMMARY AND CONCLUSIONS

The Goodenough Draw-a-Man Test was given to 325 Indian children of the Sioux, Hopi, Zuni, Zia, Navaho, and Papago tribes in nine communities, in the age group 6 through 11. The subjects were selected so as to represent their communities, and the sampling procedure appears to have been satisfactory in at least five of the nine communities. The results show the Indian children to be superior to white children. Average IQ's range from 117 (Hopi, First Mesa) to 102 (Sioux, Pine Ridge). Boys do significantly better than girls in the Hopi, Zuni, Zia, and Sioux groups. Intelligence quotients from the Arthur Performance Test were available for the same subjects. Correlation coefficients between Draw-a-Man and Arthur IQ's are low, except for the Papago (Topawa) group, where the correlation is about the same as that found in white groups. There is probably no change of Draw-a-Man IQ with age among the Indian children.

On the hypothesis that the Draw-a Man Test is a valid test of one aspect of general intelligence, namely, the formation of concepts based upon observation, with the aid of information about the various cultures in which the Indian children grow up, we can give a reasonable explanation for most of our findings, including the superiority of boys over girls in several tribes, and the superiority of the Indian children we studied over white children. The Indian children, and especially the boys, are stimulated to take an active interest in the world of nature, and given much opportunity to form and express concepts of natural objects, including the human body, on the basis of their own observation. Hence they should be expected to do well on a test which relies mainly upon concepts formed from observation and reported by drawing, without any necessary use of language.

From this study we may conclude that:

1. Indian children from the tribes tested do better than white children on the Draw-a-Man Test.

2. Indian boys from Pueblo groups do better than girls on the Draw-a-Man Test.

3. The evidence points strongly to the conclusion that environment affects the performance of children on the Draw-a-Man Test.

4. The validity of the Draw-a-Man Test as a test of general intelligence for use with Indian children is not estab-

lished by this study. However, the hypothesis that the Draw-a-Man Test is valid for one aspect of general intelligence, the ability to form concepts based upon observation, is supported to some extent by cultural data on several of the Indian groups.

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INCOME SATISFACTION AND INCOME ASPIRATION

BY RICHARD CENTERS AND HADLEY CANTRIL

Department of Psychology, Office of Public Opinion Research, Princeton University

Two of the many problems that arise in considering the implications of economic stratification for social psychology are: to what extent are members of various income groups content with their present earnings, and to what incomes do they ultimately aspire?

The first of these questions has received some prior consideration by Kornhauser (3, 4) in his studies of the attitudes of Chicago adults. The second has had previous attention in the work of Thomsen (6) and in the *Fortune* survey of the attitudes of youth (2). Both Thomsen and *Fortune*, however, were concerned with what young people (college and high-school students, respectively) expected to be earning at a given future date. Various opinion surveys have dealt with the question of what people considered to be a suitable income, or what minimum income they felt necessary for the maintenance of a "decent" standard of living. Income aspiration as related to present income has not, however, apparently been previously examined.

More data on both problems will obviously help us in our understanding of motivation in social life. The difficulty and hesitancy with which some people discuss so personal a matter as their earnings may have discouraged research to date, so that the present results may, at least in part, fill in a long-standing gap in the accumulation of data on socio-economic stratification.

METHOD

In a survey made by the Office of Public Opinion Research at Princeton the weekly income of respondents was ascertained as follows: the respondent was handed a card arranged according to income classes as shown below:

A. Under $10
B. Between $10 and $14.99
C. Between $15 and $19.99
D. Between $20 and $29.99
E. Between $30 and $39.99
F. Between $40 and $59.99
G. Between $60 and $99.99
H. $100 and over

The respondent was asked in which of these groups the average weekly income of his immediate family belonged. After this information was obtained, the interviewer then inquired, "About how much more money than that do you think your family would need to have the things that might make your family happier or more comfortable than it is now?"

A total of 1239 persons representing a cross-section of the national population 18 years of age or older were interviewed, and replies on these two questions were obtained from 1165. The usual data concerning occupation, age, sex, education, etc., were also recorded by the interviewers.

Though responses were not definitely solicited as to whether or not the individual was satisfied with his present income, the answer indicating satisfaction was spontaneously given in a consider-

able proportion of cases. Accordingly, this response has been included in the analysis of the results. Not all of the individuals who indicated that they were dissatisfied and wanted more specified a definite amount desired, hence in calculating the "increase wanted" these individuals are not included, though they are represented in the figures indicating percentage dissatisfied.

For each individual, the additional amount desired has been translated into a percentage which represents a ratio of the additional amount desired to present income. A mean of these individual percentages appears as the "increase wanted" for each group. It should be recognized in interpreting the data that a decrease, for example, in the "increase wanted" does not mean necessarily that the individual desires *less money*. An individual with a present income of $30, for example, who wanted 100 per cent more would desire $30 more, while an individual whose present income was $60 and who wanted only 50 per cent more would desire an identical sum. The "increase wanted" is always in relation to present income. Except in Table 1, the percentage terms have been adhered to throughout the report. In this instance "increase wanted" has also been translated into dollar-and-cents terms in order to make an additional and significant comparison to be considered below.

Finally, it should be observed that the occupational classification presented is based upon the reported occupation of the *head* of the respondent's family. The large number of unclassified occupations represent mainly the wives of servicemen, housewives, widows, and those whose occupational status was generally ambiguous.

RESULTS AND CONCLUSIONS

1. *Over one-half of the population is dissatisfied with its present income, and a large increase in income is generally desired by those who are dissatisfied* (Table 1).

Less than one-third (32 per cent) of the people are satisfied, and those dissatisfied want, on the average, an 86-per-cent increase over their present income.

2. *The higher a person's income is, the more likely is he to be satisfied with it and the smaller is the PROPORTION of present income desired in addition, except in the case of persons in the upper income group where those who are dissatisfied want a relatively large increase* (Table 1).

The decline in the percentage more wanted is quite regular until the group making $100 and over is reached, but here a large discrepancy occurs. This discrepancy might at first be thought attributable to the smallness and possible lack of representativeness of our sample at this level, for the number wanting a specified amount more is considerably attenuated because of the comparatively large number of satisfied. A closer examination of the cases in this category reveals, however, that this large increase is due to the presence in this group of a considerable proportion of professional people, and that it is they who want the relatively large increases. Our data do not provide any direct evidence as to why this is so, but they suggest that it is because at this level physicians, lawyers, and college professors are competing for social status with persons far above them in income, so that the relatively large increases desired represent a wish to strengthen an already high social status with a correspondingly high income status. In a previous study on the social

and economic class identifications of various occupational groups by Wallace, Williams, and Cantril(7) it was found that there existed a large discrepancy between the two identifications for professional people. In their study, while 11 per cent of professional people identified themselves as upper class socially, only 5 per cent so classed themselves economically. Another pertinent finding is that of Smith (5), who reports that inferiority feelings are more prevalent among the children of professional people than among those of any other group.

3. *For those who are dissatisfied, it is generally true that the more money a person has the more money he wants* (Table 1).

Though there is a decrease in *relative* income aspiration as income increases, the *absolute* income aspiration increases with income. It is clear that those in the lower income brackets have much lower *absolute* aspiration levels than those of relatively higher income status, but in terms of their present earnings their aspiration levels are *relatively* higher. The significance of this discovery for individual satisfaction and discontent is plainly evidenced in the differential satisfaction and dissatisfaction percentages of the various groups.

Individuals in the lowest income group do want a great deal more than they are now getting, but, in comparison to the sums wanted by those above them, theirs is a modest want indeed. An individual's present earnings obviously provide him with a frame of reference by means of which he sets his aspirations and judges his needs.

4. *By and large, income is a more reliable index to satisfaction and desire than occupation is. There is, however, a tendency for persons in those occupations providing greater personal independence to be more satisfied in all income levels.*

Satisfaction and aspiration vary in a systematic fashion with income, and to a certain extent with occupation when either variable is considered alone, but when income is held constant and occupational comparisons again made, income retains its differences in a consistent direction, while occupation does not.

TABLE 1

INCOME SATISFACTION AND INCOME ASPIRATION

WEEKLY INCOME	CASES	DISSATISFIED	SATISFIED	NO OPINION	NO. SPECIFYING AMOUNT MORE WANTED	INCREASE WANTED IN PERCENTAGES	INCREASE WANTED IN DOLLARS*
National	1165	56%	32%	12%	581	86%	$.....
Under $20	163	68%	16%	16%	100	162%	16.20
$20–$29.99	170	72%	19%	9%	116	97%	24.25
$30–$39.99	207	67%	20%	13%	129	66%	23.10
$40–$59.99	310	54%	35%	11%	147	59%	29.50
$60–$99.99	191	43%	49%	8%	73	52%	41.60
$100 and over	124	20%	66%	14%	16	100%	100.00

* The dollars-and-cents "increase wanted" is a translation based on the percentage indicated—the mid-point of the income group concerned was used to construct this estimate. Ten dollars is used as the mid-point for the "under $20" group, while for the "$100 and over" group, no mid-point could be assigned, and thus $100 was used as the basis for calculation.

There is, however, a tendency for business men and farmers to be relatively well satisfied, irrespective of the income group in which they are classified. These same persons also tend to ask for relatively less additional income.

TABLE 2

COMPARISON OF OCCUPATIONAL GROUPS IN INCOME SATISFACTION AND ASPIRATION

OCCUPATION	CASES	DISSATISFIED	SATISFIED	NO OPINION	NO. SPECIFYING AMOUNT MORE WANTED	INCREASE WANTED
Business	77	34%	58%	8%	22	70%
Professional and semi-professional	119	48%	41%	11%	51	78%
White collar	235	53%	34%	13%	111	79%
Farmers	202	52%	35%	13%	94	75%
Skilled and semi-skilled	289	60%	30%	10%	154	72%
Unskilled	121	69%	21%	10%	78	86%
On Relief and Old Age Assistance	32	75%	12%	13%	22	178%
Unclassified	94	..	..	..	..	..

TABLE 3

COMPARISON OF OCCUPATIONAL GROUPS IN INCOME SATISFACTION AND ASPIRATION WITH INCOME HELD CONSTANT

WEEKLY INCOME AND OCCUPATION	CASES	DISSATISFIED	SATISFIED	NO OPINION	NO. SPECIFYING AMOUNT MORE WANTED	INCREASE WANTED
Under $30						
White collar	56	71%	20%	9%	36	118%
Skilled and semi-skilled	56	82%	9%	9%	44	113%
Unskilled	60	70%	17%	13%	40	107%
Farmers	80	59%	23%	18%	43	98%
On Relief and Old Age Assistance	32	75%	12%	13%	22	178%
$30–$59.99						
Semi-professional	57	61%	30%	9%	33	65%
Business	14	57%	43%	0%	8	82%
White collar	87	60%	22%	18%	46	64%
Skilled and semi-skilled	178	57%	32%	11%	89	57%
Unskilled	61	67%	26%	7%	38	63%
Farmers	92	55%	35%	10%	46	58%
$60 and over						
Professional and semi-professional	62	35%	52%	13%	18	101%
Business	59	27%	63%	10%	12	47%
White collar and managerial	92	36%	54%	10%	29	55%
Skilled and semi-skilled	55	40%	49%	11%	19	54%
Farmers	30	23%	70%	7%	5	44%

It seems reasonable to account for this because of the greater personal freedom, security, and independence of these two groups.

Certain other uniformities also occur. For example, in all three groups, comparisons are available for the white-collar, skilled and semi-skilled labor, and farmer categories. The white-collar workers are consistent in wanting more than these other two groups, though they are not so consistently the least satisfied of the three.

TABLE 4

COMPARISON OF EDUCATIONAL GROUPS IN INCOME SATISFACTION AND ASPIRATION WITH INCOME HELD CONSTANT

WEEKLY INCOME AND EDUCATION	CASES	DISSATISFIED	SATISFIED	NO OPINION	NO. SPECIFYING AMOUNT MORE WANTED	INCREASE WANTED
Under $30						
Grammar School	198	73%	15%	12%	132	123%
High School	102	67%	21%	12%	62	134%
College	30	70%	27%	3%	20	120%
$30–$59.99						
Grammar School	180	58%	32%	10%	90	58%
High School	224	59%	29%	12%	121	63%
College	112	63%	25%	12%	66	65%
$60 and over						
Grammar School	44	32%	52%	16%	10	31%
High School	118	37%	59%	4%	37	50%
College	152	32%	55%	13%	41	80%

5. *An individual's present income is a more reliable index to his present satisfaction and desire than education.*

Since it is known that education correlates positively with economic status, it is not surprising to find that satisfaction increases and the "increase wanted" goes down as one goes up the educational scale. However, Table 4 shows that dissatisfaction clearly varies with the income group, rather irrespective of education within each income group.

For the minority earning over $60 a week, it is significant to notice that, although there is little variation by education in the percentage satisfied, there are sizable differences by education in the "increase wanted" by those who do say they are dissatisfied. This is further evidence of a previous finding that "the higher the *social* class, the more is it likely to be founded on non-economic criteria" (1). Persons with relatively high education and with incomes that are high in relation to the population as a whole are those most likely to come into close social contact with individuals far above them economically, compared to whom they feel quite poor, but with whom they tend to identify themselves.[1]

SUMMARY

A national representative sample of the adult population 18 years of age or older has been examined to determine

[1] Age differences with respect to satisfaction with present income are insignificant when economic status is held constant. The only difference of any magnitude occurring between the older and younger groups is in the "increase wanted" at the upper income level. This is again attributable to the presence here of the small professional group previously discussed whose aspiration level also appears to increase with age.

the status of present contentment and desire with respect to income. The higher an individual's income is, the more likely he is to be satisfied with it. However, if a person is dissatisfied, then the *absolute* amount of money he wants in addition increases with income, although the *relative* amount more wanted goes down.

Occupational differences exist but are generally subordinate in importance to income differences. The "lower" a person's occupational status, the more likely he is to be dissatisfied with his present income and the more likely he is to want a proportionately larger increase over his present earnings, although certain exceptions occur in occupations that provide greater personal freedom.

Educational differences are also indicated but are likewise subordinate to income differences. Satisfaction and dissatisfaction vary with income quite irrespective of education within the income group, except for the minority earning over $60 a week where persons with high education who are dissatisfied want significantly more than others in their economic group.

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SHORT ARTICLES AND NOTES

A NEW METHOD FOR STUDYING DISORDERS OF CONCEPTUAL THINKING

BY HAROLD RASHKIS, JANE F. CUSHMAN, AND CARNEY LANDIS
Psychiatric Institute, Columbia University

STUDIES by Bolles, Goldstein, Vigotsky, Weigl, Hanfmann and Kasanin, and others have shown that the loss of the ability to generalize and use abstract methods of thinking is among the more significant mental changes found in certain psychopathological conditions. This loss is apparently not the same in the various mental diseases. Indeed, there is good reason to believe that there is considerable variation in the ability to think in abstractions among patients suffering from a single variety of psychosis, or even in the same patient from week to week. In view of the difference between the primary symptoms of schizophrenia and of general paresis, it seemed worthwhile to investigate further the difference between the schizophrenic and the paretic in their ability to conceptualize.

In the present study we have compared the behavior of schizophrenics, paretics, and normal subjects with respect to abstract, complex, and concrete behavior. Certain of the terms which we use may be defined as follows. *Abstract behavior* denotes the voluntary forming of an attitude wherein the subject is actively able to grasp the essential aspects of a new situation, to behave in accordance with his attitude, and to account satisfactorily for his behavior. *Complex behavior* describes the fact that the subject selected terms representing aspects of a possible situation without being able to account for his selection. *Concrete behavior* denotes the inability of the subject to grasp essential relationships, to arrange new material in a conceptual scheme, or to relate the aspects of a new situation with regard to his own personal experience or sensory preference. In comparing the behavior of the groups, the terms *volitional activity* and *passive acceptance* are used, the former meaning an active search for principles governing the test situation, and the latter referring to the acceptance of the situation in terms of familiar sensory impressions.

METHOD AND PROCEDURE

This method differs from the usual type of sorting test in that words instead of substantial figures or objects are the material to be sorted. It was thought that words, by reason of their association value, might lead to more subtle distinctions of grouping than the dichotomous possibilities of the more familiar object-sorting tests.

NEED MONEY	WORKING	JOB	INDUSTRY
SEX DESIRE	SEX ACT	WOMAN OR MAN	MARRIAGE
HUNGRY	EATING	FOOD	RESTAURANT
THIRST	DRINKING	LIQUID	FOUNTAIN

DIAGRAM 1

On a sheet of heavy cardboard was drawn a 6-inch square, which was, in turn, subdivided into 16 equal smaller squares, four on a side. In four of the squares, those extending diagonally from the lower left to the upper right-hand corner, were printed the words:

THIRST, EATING, WOMAN, and *INDUSTRY*. (See diagram.) (When the test is given to a female subject, the word *MAN* is substituted for *WOMAN*.)

On 12 1-inch cardboard squares were printed the word or words: FOOD, JOB, WORKING, SEX ACT, DRINKING, FOUNTAIN, MARRIAGE, LIQUID, RESTAURANT, HUNGER, NEED MONEY, and SEX DESIRE.

The subject (S) was seated at a desk, on which was placed the diagram. The cardboard squares were scattered carelessly before him. At the side, and behind S, sat the examiner (E).

If S began to sort the cardboard squares into the empty spaces on the diagram, E made no comment. If S appeared puzzled by the situation, or if he asked E what was expected of him, E replied, "You are to sort the cardboard squares in the spaces where you think they belong." If S covered one of the key words (*i.e., THIRST, EATING, WOMAN, INDUSTRY*), E pointed out that there are 12 empty spaces and 12 cardboard squares.

This phase of the test was termed *Sorting into complexes*. In order to *pass* this portion of the test it was necessary for S to sort—either horizontally or vertically—in a column with the cue word the three cardboard squares bearing terms most closely related to each of the four cue words. With *THIRST*, then DRINKING, FOUNTAIN, and LIQUID were to be associated; FOOD, RESTAURANT, and HUNGER with *EATING*; SEX ACT, MARRIAGE, and SEX DESIRE with *WOMAN* (or *MAN*); JOB, WORKING, and NEED MONEY with *INDUSTRY*. The limited number of cardboard squares and empty spaces formed a circumstance sufficiently coercive to impress all but the most deteriorated subjects. If S was unable to perform the sorting into complexes, E then organized the groups for him, and questioned S as to the reason why they *go together*. If S merely passively accepted the arrangement, E transposed two of the terms, *e.g.*, FOOD and LIQUID, and, if this arrangement was still acceptable to S, his solution was graded as unsatisfactory.

When S had spontaneously completed the sorting, he was asked to verbalize his behavior, and this response was noted by E. This concluded the first phase of the test.

S was next asked if he could sort *differently*, being cautioned not to destroy the horizontal (or vertical, as the case may be) *complexes* during the rearrangement. S was told to look for some *general principle* that concerned all of the four columns. *E noted carefully any basis for organization that was suggested by S.* If, however, no general principle was forthcoming, E instructed S to select the word that went best with *THIRST* in each of the three remaining groupings. These terms are, of course, HUNGER, SEX DESIRE, and NEED MONEY. If S was unable to select these terms, his alternative choices were noted, and E volunteered the required grouping. S was then asked to verbalize the basis for the organization, the obvious response being: "They're all desires!"

After the first of the *abstract* groupings had been established, S was asked to arrange the remaining three columns in a similar fashion. He was asked to verbalize his arrangement. If, after prompting by E, S was unable to group abstractly the remaining three columns, E sorted the terms as in Diagram 1. S was then asked to explain what E had done. If there was no comprehension, E suggested classifications as follows: Column I, Desire; II, Activity whereby the desire might be satisfied; III, Object for satisfaction; and IV, Institution for the satisfaction of the desire.

Acceptance of these classifications by S did not always signify presence of the abstract attitude. Various checks were required to show that the acceptance was not merely passive or a result of E's suggestions. E would transpose two of the terms, *e.g.*, JOB and WORKING, and ask S if this arrangement were still satisfactory. Or E would ask S to explain the classification in his own words. If S was then unable to recall the classification, it was concluded that he had no active understanding of E's terms.

It is to be noted that in the course of the test there are two distinct shifts that were required of S. The first of these shifts was made by S when he sorted the terms into complexes instead of considering them as specific terms or as concrete things. The

basic concrete attitude was shown when S merely paired the terms, or grouped them into interrelating or incoherent spheres. The second shift occurred when S superimposed on the complex groupings a sorting in terms of an abstract principle.

It was not necessary for a subject to pass consecutively through all three levels of behavior. Many S's would assume immediately a complex or an abstract attitude. In such instances the shift was considered as implicit, as the complex groupings include concrete spheres, and the abstract sorting depends on the complexes.

RESULTS

The test was given to 36 subjects: 9 normal adults aged between 16 and 21 years, 9 normal children aged between 13 and 15 years, 8 schizophrenic patients, and 10 general paretic patients. No attempt was made to partial out intelligence. Normal adults closely matched the school educational achievement of the schizophrenics, while the normal children's closely approximated that of the paretics.

Individual Differences. Table 1, in the form of a scatter diagram, shows the highest level of behavior achieved by the various subjects. The various levels of behavior listed are ordered inversely with the amount of prompting given by E, or directly with the degree of spontaneity shown by S in shifting from a lower to a higher level. The most abstract type of behavior possible with this method was shown by one normal subject. When confronted with the test situation R. S. immediately sorted abstractly and verbalized adequately, requiring no prompting. The other extreme of behavior was displayed by G. L., a paretic. When instructed to sort in the empty spaces, G. L. picked up the card bearing the word LIQUID, denied that it went with anything else, and began to babble incoherently about "liquid" and "liquor," which terms were used interchangeably. He did not accept the complexes, removing LIQUID from the form board and clutching it in his hand as he continued his unintelligible oration.

Most of the subjects' behavior fell well within these two extremes. Complex behavior, which was shown by the greatest number of subjects, was well illustrated by one of the children, J. L., who said when questioned, "When you're walking along you see a fountain and get thirsty, then you go in and get a drink." In such instances E attempted to induce an abstract level by a method of simple analogies, but only a "pseudo-conceptual" level (*cf.* Vigotsky) was reached.

Group Differences. It is apparent from Table 1 that the experimental groups tended

TABLE 1

BEHAVIOR OF DIFFERENT GROUPS ON WORD-SORTING TEST

The group differences are significant by the Chi-square method (p=.01 or less) in all except the comparison between schizophrenics and normal children.

TYPE OF BEHAVIOR SHOWN	NORMAL ADULTS	SCHIZOPHRENES	NORMAL CHILDREN	PARETICS	TOTAL
Abstract					
Sorted by S	2	0	0	0	2
Sorted by E	6	1	0	0	7
Complex					
Sorted by S	1	5	9	3	18
Sorted by E	0	2	0	0	2
Concrete					
Interrelating spheres	0	0	0	5	5
Cue for conversation	0	0	0	2	2
Total	9	8	9	10	36

toward certain characteristic levels of behavior. Normal adults were able to assume the abstract attitude, children behaved on a complex level, as did most schizophrenics, while paretics were generally unable to attain even the complex level. To make the comparison in terms of *lower* and *higher* would be to say that schizophrenics think like children, and paretics, by extrapolation, like infants, which is obviously an oversimplification. Table 2 attempts to show qualitative differences and similarities between the groups in terms of the degree of volition (an integral part of the abstract attitude as described by Goldstein and Scheerer) shown by the individual subjects. When considered in this light, the schizophrenics appear most like normals, while it is the paretics who seem to behave like the children. Consideration of a few protocols may clarify this suggestion.

TABLE 2

DEGREE OF VOLITION SHOWN BY SUBJECTS

INDEPENDENCE RATING	NORMAL ADULTS	SCHIZOPHRENES	NORMAL CHILDREN	PARETICS
Abstract principle offered by S or Abstract principle offered by E rejected by S	6	3	0	0
	0	4	0	0
Accepted E's terms or Showed passive acceptance	2	0	0	0
	1	1	9	10

S. L. exemplifies the schizophrenic whose performance would conventionally be regarded as no higher than the level of complex behavior, yet who offered an abstract principle for what seemed to be a concrete organization. Arranging the terms of interrelating spheres, a concrete form of behavior, he explained his basis for behavior as follows: "I tried to weight the stronger against the weaker sequences. I tried to wake up in my memory parallel trends concerning the subconscious hope for woman as the mainspring of aspirations." The parallel trends he described both in a constructive sense and in terms of a desire existing from childhood. These two trends he explained as being in indirect conflict with each other, and he described *INDUSTRY* as "an outgrowth of periodic freedom from slavery to sex."

In marked contrast with the eccentric behavior of S. L. was the passive acceptance shown by L. B., a paretic. He pointed out the words one by one, giving for each a definition or a description (*e.g., THIRST*—"hunger for a drink"). With prompting, he was able to sort NEED MONEY, WORKING, and RESTAURANT, explaining how he had worked in a restaurant for six months, when he had made $22 per week.

The tendency toward relating personal incidents was marked in the paretics. Most common was talk about money, their family, and personal abilities. The children, too, showed similar activity. One boy picked up MARRIAGE and said: "That's my sister!"

CONCLUSIONS

Except for one atypical instance, the schizophrenics, paretics, and children were found unable to assume a categorical or abstract attitude. The clear difference between the groups was found in the degree of volition and in the presence or absence of a governing or determining principle of behavior. The paretics showed a minimum of the former and an absence of the latter. The schizophrenics showed both volition and some determining basis for their behavior, either in the form of a positive abstract principle, or, negatively, in the rejection of E's concepts.

It is plain that the paretic has deteriorated to (or below) the level of the pre-adolescent, both in regard to the complexity of his thinking and in the passive acceptance shown by his behavior. Of the schizophrenic it is not adequate to say that he had lost the abstract attitude and now thought in complexes on the level of children.

There is no doubt that schizophrenics do not deal with abstract concepts in the normal manner, but this does not necessarily mean that they do not have concepts of their own which serve as bases for behavior, even as the categorical attitude may guide the behavior of normals. The difference, of course, lies in the rigidity and eccentricity of the schizophrenic. Normals generally recognize the same abstract concepts, while no two schizophrenics apparently follow the same principles. There is no logic in the thinking of the paretic. Of the two groups of abnormals studied, only the paretic may be truly said to have lost the ability to make abstractions. The schizophrenic can think in an abstract way but it is an eccentric or disorganized performance.

SUMMARY

1. A sorting test, in which verbal units were to be sorted into complexes and then into an abstract arrangement, was devised and given to a group of normal adults and normal children, a group of paretic patients, and a group of schizophrenic patients.

2. Normal adults alone were able to sort on an abstract or categorical level. Both schizophrenics and children were able to sort in terms of complex organizations, while paretics were generally unable to attain any complexity in their sorting.

3. Children and paretics showed little volitional activity, but schizophrenics showed volition similar to normal adults.

4. Categorical behavior of schizophrenics is differentiated from that of normals by its eccentric nature and by its resistance to shifting from one attitude to another.

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A COMPARISON OF PSYCHIATRIC RATINGS WITH MINNESOTA MULTIPHASIC PERSONALITY INVENTORY SCORES

BY ARTHUR L. BENTON, LIEUT. COMDR., H(S), USNR
AND
KATHRYN A. PROBST, LIEUT., W(H), USNR
U. S. Naval Training Station, Newport, Rhode Island

MANY of the recent reports on the use of the Minnesota Multiphasic Personality Inventory (2, 3) have dealt with its efficacy as a screening device in personnel selection situations. However, since one of the unique features of the Inventory is its provision for the independent assessment of certain definite dimensions of personality, studies directly investigating the characteristics and validity of the separate scales are desirable. This is the purpose of the present study and a previous investigation by one of the writers (1), which are concerned with the analytic-diagnostic features of the Inventory rather than with its application as a general screen test.

The present study investigates the degree of agreement between ratings by psychiatrists on their patients with respect to the traits or trends measured by the Minnesota Inventory and the Minnesota scores on these trends. Since neither the psychiatric ratings nor the test scores should be considered as representing a standard or "true" measure of the trends in question against which the validity of the other measure can be assessed, the comparison is not a simple "validation" of either measure. Its purpose is to note the areas of relative agreement and disagreement and to discuss the possible significance of the findings with a view toward enhancing the value of the clinical application of the Inventory.

METHOD

Four naval psychiatrists, who had been given the *Minnesota Manual* (2) for reference, were requested to rate their patients on the trends investigated by the Minnesota Inventory. The selection of patients for rating was made by the psychiatrists. It was understood that the psychiatrists would base their ratings of each trend, *as it was defined in the Minnesota Manual*. The psychiatrists were asked to place a patient in one of four categories with respect to the strength of each trend. The four categories were presented graphically in box form to the rating psychiatrists and were labeled as follows:

1. AVERAGE TREND (T-score 60 or below)
2. SOMEWHAT STRONGER THAN AVERAGE BUT WITHIN NORMAL LIMITS (T-score 61–70)
3. DEFINITELY STRONGER THAN AVERAGE, POSSIBLY PATHOLOGICAL IN SIGNIFICANCE (T-score 71–79)
4. PATHOLOGICAL STRENGTH OF TREND (T-score above 79)

After a rating was made by a psychiatrist, a patient thus rated was given the Minnesota test. Test papers with "?" or "L" scores above 66 were rejected as being of questionable validity. When an "F" score of 64 or more was made, the questions contributing to the high "F" score were personally reviewed with the patient to determine whether he fully understood the statements on the cards. If it appeared that the high score was due to faulty comprehension of the statements, the test was rejected but if the high score apparently represented a genuine individuality in attitude the test results were considered to be valid.

RESULTS

Seventy-six patients were rated by the psychiatrists and subsequently received the Minnesota test. Of these 76 patients, six showed high "F" scores indicative of faulty comprehension of the test items and the test papers of these men were rejected, leaving 70 sets of papers for final consideration. The results of the psychiatrists' ratings with respect to

each trend were placed in a dichotomous classification, the ratings of "average" and "somewhat stronger than average but within normal limits" being placed into a broad normal group and the ratings of "definitely stronger than average, possibly pathological in significance" and "pathological strength of trend" being placed into a broad abnormal group. The mean test scores of the normal and abnormal groups were computed and compared. The results are presented in Table 1.

TABLE 1

MINNESOTA TEST SCORES OF PSYCHIATRICALLY RATED NORMAL AND ABNORMAL GROUPS

TREND	NORMAL				ABNORMAL				CR
	No. CASES	MEAN	SD	σ_M	No. CASES	MEAN	SD	σ_M	
Hypochondriasis	53	73.1	12.6	1.7	17	79.4	15.4	3.7	1.5
Depression	56	76.2	17.7	2.4	14	85.6	15.7	4.2	1.9
Hysteria	43	70.8	11.2	1.7	27	71.6	9.4	1.8	.3
Psychopath	58	61.0	14.1	1.9	12	75.8	18.5	5.3	2.6
Femininity	61	52.4	10.0	1.3	9	59.8	14.2	4.7	1.5
Paranoid Trend	65	58.2	13.9	1.7	5	73.0	9.7	4.3	3.2
Psychasthenia	51	68.9	15.0	2.1	19	72.2	18.6	4.2	1.3
Schizophrenia	55	66.0	19.3	2.6	15	81.0	15.2	3.9	3.2
Hypomania	69	58.4	11.6	1.4	1	52			

It will be noted that in the case of the *Psychopathic Deviate, Paranoia,* and *Schizophrenia* trends the differences with respect to mean test score between the normal and the abnormal groups can be considered to be statistically significant. In other words, there is a significant degree of agreement between the psychiatrists and the test in regard to the strength of these trends in patients.

On the other hand, there is no substantial amount of agreement with respect to the strength of the *Hypochondriasis, Depression, Hysteria, Femininity,* and *Psychasthenia* trends, although in all cases the mean test scores of the psychiatrically rated abnormal groups are higher than those of the normal groups. Especially noteworthy is the complete lack of agreement with respect to the strength of the *Hysteria* trend.

DISCUSSION

Since the findings indicate that the degree of agreement between test scores and psychiatrists' ratings varies significantly with the different trends, each trend merits separate discussion. Possible reasons for the lack of agreement with respect to certain trends may be considered. In addition, the results may be evaluated in the light of a recent study by one of the writers (1) in which the validity of some of the Minnesota scales was investigated by administration of the test to patients whose diagnoses were established with practical certainty.

Hypochondriasis. The lack of agreement with respect to this trend is based essentially on the fact that, while the test scores of the psychiatrically rated abnormal group are high, those of the normal group are almost equally high. In other words, the Minnesota test reports overconcern with bodily processes and symptoms in cases in which the strength of this tendency has not impressed the psychiatrists. It is probable that in these cases there is in fact a stronger-than-average concern with somatic symptoms, which the Minnesota test accurately reports. The psychiatrist, his attention focused on what he considers the more basic problems, may tend to minimize the strength of the "hypochondriacal" trend in certain patients. It is also possible that in some cases the psychiatrist made allowances for a stronger-than-average somatic concern because of the existence of minor physical pathology in the patient. In the validity study (1) to which reference has been made, it was found that patients suffering from cardiovascular disease tended to make high

scores on the *Hypochondriasis* scale of the Minnesota test. In these patients the stronger-than-average concern with bodily processes, reflected in these high scores on the *Hypochondriasis* scale, cannot justifiably be interpreted as evidence of "true" hypochondriasis, i.e., *excessive* concern over bodily processes and exaggeration of minor complaints with no substantial pathological basis. In view of their physical condition, the stronger-than-average concern of these patients seems to be quite appropriate. It would appear, then, that in measuring, probably with considerable accuracy, the extent of concern over somatic processes and symptoms, the *Hypochondriasis* scale is disclosing a personality trend which is not necessarily truly hypochondriacal in nature and which may have a sound basis in physical pathology. Certainly, the rather summary statement in the *Minnesota Manual* with respect to the *Hypochondriasis* scale that it "detects a difference between the organically sick person and the hypochondriac" (2, p. 4) requires further investigation before it can be accepted.

Depression. As is the case with the *Hypochondriasis* scale, both the normal and abnormal groups show high "depression" scores. The mean score (86) of the abnormal group is 10 points higher than the mean score of the normal group but the difference is not statistically significant. Personal experience with patients who have taken the test and who have made high "depression" scores suggests that the Minnesota test measures a feeling of unhappiness, a sense of dissatisfaction, pessimism in outlook, and a "dysphoric" mood in general. On the other hand, the psychiatrist's concept of depression appears to depend to a great extent on objective manifestations of the depressed state, e.g., weeping, retardation and poverty in response, apathy, and slovenliness. In short, the test is primarily concerned with the patient's reported attitudes and feelings, while for the psychiatrist these reported attitudes and feelings must be reinforced by appropriate behavioral characteristics to establish the impression of a true depressive state.

It may be noted that these proposed explanations of the discrepancies with respect to both the *Hypochondriasis* and the *Depression* trends between the psychiatric ratings and the test scores (i.e., that the Minnesota test probably identifies the true hypochondriacs and depressives but in addition labels patients who do not belong in these categories as "hypochondriacal" and "depressive") are quite compatible with the assumption that these scales were originally carefully validated with selected cases in these diagnostic categories. It is not that the Minnesota test fails to identify true hypochondriacs and depressives, but it does seem to give abnormally high scores with many non-hypochondriacal and non-depressive patients. It yields too many "false positives," to use the jargon of the laboratory.

Hysteria. In the case of this trend, no agreement whatever between the psychiatric ratings and the test scores appears. We know of no ready explanation for this lack of agreement. It may be noted that in the validity study (1) the *Hysteria* scale was found to be relatively unsuccessful in identifying established cases of conversion hysteria.

Psychopathic Deviate. A clear agreement approaching statistical significance (CR = 2.6) between the ratings and the test scores is found in the case of this trend. The mean test score (61) of the psychiatrically rated normal group is definitely within normal limits, while the mean score (76) of the abnormal group is quite high. In the validity study (1), the *Psychopathic Deviate* scale was found to identify delinquent psychopaths with reasonable accuracy, correctly classifying over four-fifths of the men tested. Of the 58 cases rated by the psychiatrists as normal with respect to this trend, 9 had scores of 75 or more. It is quite possible that, in the absence of a delinquent history or clearly expressed asocial and amoral attitudes, the existence of a strong trend of this type in a patient could be overlooked by an examiner. In these cases the Minnesota test might disclose the full strength of this trend and thus identify "pre-delinquents" or "proto-delinquents" who might otherwise escape attention.

Femininity. In the case of this trend, both the normal and abnormal groups achieve mean test scores well within normal limits. This result is in direct contrast to the findings with the *Hypochondriasis, Depression,*

and *Hysteria* scales, on which both the normal and abnormal groups made abnormally high mean test scores. It is evident that in general the psychiatric rating of especially strong feminine tendencies in a patient is not reflected in a high test score. In this connection it should be noted that the validity study(1) showed that confessed male homosexuals with originally high scores on the *Femininity* scale of the test were usually able to bring their scores down to well within normal limits when requested to attempt to conceal the fact of their homosexuality. It may be, then, that the *Femininity* scale, dependent as it is on the honesty and frankness of the subject, is frequently incapable of disclosing a feminine trend apparent to the interviewing clinician.

Paranoia. In the case of this trend, the ratings and the test scores show an excellent degree of agreement. The mean test score (58) of the psychiatrically rated normal group is well within normal limits, while the mean score (73) of the abnormal group is excessively high. Nevertheless, seven of the 65 patients rated as normal with respect to this trend by the psychiatrists made scores of 80 or more. As an explanation of this finding, the possibility is advanced that the test scale is capable of bringing to the surface latent paranoid tendencies in the form of suspicious, distrustful basic attitudes not easily elicited in interview.

Psychasthenia. No significant degree of agreement between the psychiatric ratings and the test scores is shown in the case of this trend. Both the normal and abnormal groups show high mean test scores.

Schizophrenia. An excellent degree of agreement is shown between the psychiatric ratings and the test scores. The mean test score (66) of the psychiatrically rated normal group is rather high but within broad normal limits. The mean test score (81) of the abnormal group is extremely high, far beyond normal limits. The validity study (1), in harmony with the original findings of Hathaway and McKinley (2), indicated that the *Schizophrenia* scale was not effective in identifying patients suffering from gross schizophrenic disorders, correctly classifying only about half of these individuals. The schizophrenes included in the present study were for the most part *schizoid psychopaths* rather than psychotic patients. While showing evidence of seclusiveness, inflexibility, and undeveloped paranoid trends, they did not exhibit the intellectual impairment, delusional thinking, or behavioral deterioration generally characteristic of the psychotic schizophrene. All were on open wards. Of the 55 patients who were rated as normal with respect to this trend by the psychiatrists, 15 had scores of 75 or more. As with the *Paranoia* scale, it may be that the *Schizophrenia* scale is especially sensitive in eliciting the schizoid trend in psychopaths of this type and thus will disclose the trend when it eludes the interviewing clinician. If this is so, the scale can serve as a valuable adjunct in personality evaluation.

SUMMARY

A study of the relationships between ratings by psychiatrists of patients with respect to the personality trends investigated by the *Minnesota Multiphasic Personality Inventory* and the test scores themselves yielded the following results:

1. There was a significant degree of agreement between the psychiatric ratings and the test scores with respect to the *Psychopathic Deviate, Paranoia,* and *Schizophrenia* trends.

2. There was not a significant degree of agreement between the psychiatric ratings and the test scores with respect to the *Hypochondriasis, Depression, Hysteria, Femininity,* and *Psychasthenia* trends.

The significance of the findings is discussed.

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A FOLLOWUP STUDY OF NEUROPSYCHIATRIC SCREENING *

BY CECIL L. WITTSON, COMDR., MC, USNR, WILLIAM A. HUNT, COMDR., H(S), USNR
IRIS STEVENSON, LT. (JG), H(W), USNR

WHILE many articles have been written on neuropsychiatric selection in the military services, there has been little followup material published on the results of such selection. The present article summarizes the subsequent neuropsychiatric history of those recruits who completed their training and were passed on to the fleet from the U. S. Naval Training Station, Newport, Rhode Island, during the month of September, 1941.

The specific procedures in use at Newport have been described in detail elsewhere.[1] A summary will suffice at this point. Every recruit entering the Naval Training Station at Newport received a brief psychiatric interview as part of the medical examination given him upon his arrival at the station. This interview averaged between two and three minutes per man and was conducted by a psychiatrist. As a result of this examination, those men who were considered to be fit and to show no signs of psychiatric symptomatology were immediately passed on to duty. Those men who were judged to be unfit for neuropsychiatric reasons were admitted to an observation ward for further detailed study, as a result of which they were either separated from the Service or returned to duty. Men in whom some psychopathic tainting was suspected but not detectable to a degree sufficient to warrant admission to the observation ward were sent on to trial duty and reexamined at a later date after a social history had been obtained. By the time the recruits had completed their basic training, all men believed to be neuropsychiatrically unfit were separated from the Naval Service. The others, including those suspects who exhibited minimal but not disqualifying symptomatology, were passed on to the fleet as fit for duty. Although psychiatric selection many times has had to function under seriously handicapping conditions of insufficient personnel and inadequate facilities, it was the opinion of the staff that during September, 1941, the neuropsychiatric selection procedures at Newport were functioning adequately and efficiently. Therefore this month was selected for an experimental study of the efficiency of the selection measures.

The ideal experimental design would have been to use those men passed on to the fleet as fit as a control group and to allow some of the unfit, rejected recruits to have passed on to the fleet as an experimental group. Unfortunately or fortunately, depending upon whether one takes the scientific or humanitarian point of view, it was not possible to risk the personal health of these rejects or the efficiency of the military service by such an experimental set-up. As a control group it was possible to use those men who were cleared as psychiatrically fit during the original three-minute examination, but as an experimental group it was necessary to use, not the rejects, but those "suspects" who merited further investigation above and beyond the original three-minute interview but who were finally sent on to duty anyhow. If we think of unfitness as a continuum ranging from the clearly fit through the borderline cases to the clearly unfit, we will see that we have cut off one end of our continuum, the clearly unfit, and are essentially comparing the subsequent NP histories of the borderline and fit groups. Instead of asking the general question, "Does neuropsychiatric selection work?", we are asking this question, "Is it possible on the basis of a three-minute interview to differentiate men as neuropsychiatrically clear and as neuropsychiatrically borderline in such a fashion

* The opinions or assertions contained herein are the private ones of the writers and are not to be construed as official or reflecting the views of the Navy Department or the Naval Service at large.

[1] Wittson, C. L., Harris, H. I., Hunt, W. A., Solomon, P., & Jackson, M. M. The neuropsychiatric selection of recruits. *Amer. J. Psychiat.*, 1943, 99, 639–650.

that their subsequent military history will reveal group differences in psychiatric attrition rate and efficiency of performance?"

There were 227 men in the suspect group who merited further NP investigation following the three-minute interview but who were finally cleared for duty. This was the experimental group. From among the 1169 men immediately selected as fit on the basis of the three-minute interview, two randomly selected groups of respectively 227 and 233 recruits were taken as control groups.[2] There was sufficient uniformity in these two samplings to suggest that they are representative of the larger group and that there was no need to check the entire 1169 fit recruits. The medical records of the experimental and the two control groups then were checked for the incidence of medical surveys for neuropsychiatric reasons over a period of 38 months following their completion of recruit training. Table 1 shows that while the suspect group had a total of 7 discharges for an NP attrition rate of approximately 1.0 per cent per year, the two control groups each had only 2 neuropsychiatric surveys for an average yearly attrition rate of 0.3 per cent. While these differences do not quite attain statistical reliability (since the critical ratio is only 1.8, indicating that there are still 8 chances out of 100 that the difference is due to chance), there is a strong indication that the suspect group has indeed a higher degree of NP tainting than the control group. This NP tainting apparently reveals itself not only in the psychiatric discharge rate but also in the recruits' daily performance. Table 2, comparing the performance of the suspect group with that of Control Group I, shows that the suspect group has a lower percentage of ratings, a lower percentage of recruits undergoing trade-school training, and a greater number of days lost through AOL and AWOL, as well as a greater number

TABLE 1

NEUROPSYCHIATRIC DISCHARGES OVER A 38-MONTH PERIOD OF RECRUITS JUDGED "SUSPECT" AND "NORMAL" DURING THE ORIGINAL PSYCHIATRIC EXAMINATION

	NP SUSPECT GROUP N=227	"NORMAL" CONTROL GROUP I N=227	"NORMAL" CONTROL GROUP II N=233
Number medical surveys with neuropsychiatric diagnosis	7	2	2
Average yearly attrition rate (percentage)	1.0	0.3	0.3

TABLE 2

COMPARATIVE PERFORMANCE OF "SUSPECT" GROUP AND CONTROL GROUP I

	PERCENTAGE OF GROUP IN PETTY OFFICER STATUS	PERCENTAGE ATTENDING SERVICE SCHOOLS	PERCENTAGE FAILING IN SERVICE SCHOOLS	TOTAL NUMBER DAYS LOST THROUGH AOL AWOL	TOTAL NUMBER DAYS DISCIPLINARY SENTENCE
NP Suspect Group N=227	80	30	11	1155	4401
Control Group I N=227	90	59	8	636	1841

[2] Commander Herbert I. Harris and Lieutenant Commander Philip Solomon assisted in the original examinations in September, 1941.

of days sentence to military imprisonment. Again, these differences, while they do not show great statistical reliability, all fall in one constant direction.

Some indication of the efficiency of the neuropsychiatric screening outlined here may be obtained by comparing the attrition rate amongst the Newport group with that of groups from two other training stations where the neuropsychiatric screening was not at that time as well organized as it was at Newport. For this purpose groups of 238 and 249, respectively, randomly selected recruits who had passed through two other Naval training stations during the month of September, 1941, were checked for subsequent psychiatric attrition. Since both of these groups include what we have been calling "suspect" as well as "clear" recruits, in order to get a fair comparison with the Newport group it will be necessary to extrapolate to obtain a comparable attrition rate for the entire Newport population. If we assume that the two control groups are representative of the entire group of 1169 recruits adjudged fit and add to this the suspect group, we will arrive at an estimated average yearly discharge rate of 0.4 per cent for the entire group cleared through the Newport Training Station in September, 1941. Table 3 shows that the average yearly attrition rate for NP reasons for these two control groups from other stations is almost double that of the Newport group. This would furnish some basis for inferring that the selection procedures at Newport were successful in cutting down subsequent attrition rate.

TABLE 3

COMPARISON OF SUBSEQUENT NEUROPSYCHIATRIC ATTRITION RATE OVER A 38-MONTH PERIOD IN NEWPORT GROUP WITH GROUPS FROM TWO OTHER NAVAL TRAINING STATIONS

	NEWPORT N=1.396	STATION X N=238	STATION Xa N=249
Number medical surveys with neuropsychiatric diagnosis	17 *	5	6
Average yearly attrition rate (percentages)	0.4 *	0.7	0.8

* Estimated on basis of samplings in Table 1.

TABLE 4

SUBSEQUENT ATTRITION OVER 14 MONTHS OF "SUSPECT" AND "NORMAL" CONTROL GROUP OF SPECIAL ASSIGNMENT RECRUITS (PHYSICALLY HANDICAPPED)

	NP SUSPECT GROUP N=97	"NORMAL" CONTROL GROUP N=113
Number medical surveys with neuropsychiatric diagnosis	5	1
Average yearly attrition rate (percentages)	4.4	0.8

The above experiment has been repeated on a smaller group of Special Assignment men (recruits accepted for limited duty despite the presence of some physical handicap) who passed through the USNTS, Newport, Rhode Island, in September, 1943, and whose records were checked 14 months later. There were 113 men who were passed as clear in the brief examination and 97 suspects who were given further study before being sent to duty. The "clear" group shows only one NP discharge in the 14 months subsequent for an approximate yearly attrition rate of 0.8 per cent, while the "suspect" group shows 5 for a rate of 4.4 per cent. These

data are set forth in Table 4. Again the critical ratio is about 1.8, indicating a difference which is not quite significant inasmuch as there are still 8 chances out of 100 that it is due to chance. An interesting sidelight on the serviceability of such Special Assignment men admitted with physical handicaps is given by the fact that the subsequent medical survey rate for physical disability (not psychiatric in nature) is three times the physical disability survey rate for the other groups. The higher NP attrition rate among this group may well be due to the added handicap of physical disability.

One further check of the same general sort is possible. This comes by further restricting the sampling from the continuum of unfitness and selecting for study only those borderline cases which were very close to rejection. For this purpose a group of 309 recruits who had passed through the USNTS, Newport, Rhode Island, during the year 1943 were selected from amongst the "worst" of the "suspect" cases. These represented a group which were sent to duty with regret since it was found that they suffered from definite neuropsychiatric difficulties which were, however, not quite severe enough to merit separation from the Service. If our previous hypothesis is correct and it is possible reliably to select groups of men differing in the amount of NP tainting, the psychiatric attrition rate in this last group should be higher than that of our previous "clear" or "suspect" groups. This group shows 19 NP discharges in 18 months, for an average yearly rate of approximately 4.0 per cent, a rate equalled only by our special assignment "suspect" group where the neuropsychiatric picture was further colored by the addition of physical handicap. Here again we find our previous results substantiated.

The data reported above are not any ultimate and final validation of neuropsychiatric screening and should not be interpreted as such. The authors feel, however, that they do furnish some answer to two popular criticisms which have been raised against neuropsychiatric screening. The first of these is the criticism that it is impossible in a three-minute interview to make valid judgments of neuropsychiatric disabilities. The second is the criticism that the selection standards used in neuropsychiatric screening in World War II are purely arbitrary, based upon *a priori* medical prejudices and not related to real military potentiality. The results reported above indicate that it is possible by means of a three-minute interview to separate people into groups on the basis of the amount of neuropsychiatric symptomatology present and that the differences between these groups will be reflected both in differences in subsequent medical attrition rate for psychiatric reasons and in the efficiency of military performance.

SECURITY AND BREAST-FEEDING *

BY A. H. MASLOW AND I. SZILAGYI-KESSLER
Brooklyn College

It is accepted implicitly in the literature that there is a positive, straight-line correlation between length of breast-feeding in infancy and later psychological security (e.g., 5). The assumptions run that (*a*) breast-feeding is better for the child's security than bottle-feeding, and that therefore (*b*) the longer the period of breast-feeding (within culturally ordained limits), the greater will be the child's security. Actually, neither of these assumptions has ever been empirically proven. The exploratory research here reported indicates that neither of these assumptions can be accepted simply as it stands. Under certain dynamic circumstances, they may be true, but under other circumstances, they may be untrue.

METHOD

Students in psychology classes at Brooklyn College were asked to fill out a personality inventory (3, 4) which yields a quantitative estimate of the individual's feelings of conscious psychological security. The construction, validation, and reliability of this inventory are described elsewhere and need not be detailed here. Suffice it to say that various validation techniques as well as general experience with the inventory show that it is valid and reliable enough for the purposes of the present research. A high score means relative security and a low score means relative insecurity. Rapport was excellent and various follow-up experiments indicated that the students as a group filled out the inventory conscientiously and with conscious sincerity.

These same students were then asked to get from their mothers information about the number of months they were fed at the breast in infancy, supplemental feedings, when they were taken off the breast, unusual incidents, etc. These crude data showed a curvilinear relationship with security. That is, the highest security score averages were obtained by those who had never been fed at the breast and by those who had been fed at the breast more than a year. The

* We wish to thank Miss Audrey Gallin for her secretarial and clerical assistance. Our interest in this problem was stimulated by D. M. Levy's book, *Maternal Overprotection* (2). In order to save considerable space, we shall assume knowledge of this book, especially Chapter 4.

TABLE 1

MEAN AND MEDIAN SECURITY TEST SCORES FOR GROUPS FED AT THE BREAST FOR VARIOUS LENGTHS OF TIME *

	N	MEAN SECURITY SCORE	PROBABLE ERROR OF THE MEAN	MEDIAN SECURITY SCORE	PROBABLE ERROR OF THE MEDIAN
Wholly bottle-fed	42	57.3	10.5	77	13.0
Breast-fed less than 3 months	56	58.4	8.9	89	11.3
Breast-fed less than 6 months	85	41.7	7.2	62	9.0
Breast-fed less than 9 months	77	—1.9	7.6	23	9.6
Breast-fed less than 12 months	100	40.4	6.7	71	8.4
Breast-fed 12 months or more	56	58.8	8.9	80	11.3

* The probable errors are presented only as an approximation since the distribution of security test scores is not normal but markedly skewed to the high-score end. The standard deviation of the distribution was arbitrarily corrected to 100 for the purpose of these calculations.

lowest security score averages were obtained by those who had been fed at the breast for an intermediate period of time.

The whole procedure was repeated more carefully with a completely different group of students in order to check this unexpected result. Since the results were just about the same, these data have been pooled and are presented in Table 1.

RESULTS

Table 1 shows that the relation between security and length of breast-feeding is curvilinear rather than rectilinear. The highest security scores were obtained by the groups who were breast-fed little or not at all and by those who were fed at the breast for over a year, a relatively long time in our culture (2).

In order to obtain some approximate idea of the statistical significance of these data, arbitrary corrections were made for the non-normality of the distribution of security test scores. When this is done, critical ratios can be calculated. The difference between the highest and lowest average (58.8 and —1.9) is 5.2 times its probable error. The conventional criterion holds that such a difference should be at least 4 times greater than its probable error in order to be considered statistically significant. This enables us to say conservatively that it is very likely that statistical formulae suitable to our data should show these data to be significant or near-significant.

DISCUSSION

In view of the crudity of the data, a completely conservative conclusion had best be stated first. Our data do not confirm the generally held belief in a linear correlation between security and length of breast-feeding. At the very least, we may say that the question must again be considered to be an open one.

It is remarkable that, with such crude techniques, such clear differences should have emerged. Since crudeness in measure is usually a statistically attenuating influence—that is, it may be expected to obscure a "true" correlation rather than to create a spurious one—we may reasonably assume that more direct observation of actual feeding and more valid measurement of security would not only confirm this result but increase the width of the difference obtained.

However, upon closer analysis, there appear both logical and clinical grounds for considering our results not to be so surprising after all.

1. We have always affirmed that it was not so much what was actually done to the infant that was important, as the spirit in which it was done. Infants are quite sensitive to the subtle physical expressions of feelings of anxiety, rejection, or anger in the mother even though she goes through all the outward "purposive" motions of loving the child. For excellent examples, see (1). It must be reiterated that psychologists must study not gross behavior alone but also the subtle attitudes and motivations which are shown to the world by the delicate, unconsciously performed *expressive* behaviors, or movements, which may or may not be purposive in the ordinary sense.

2. Most of our students are second-generation Americans. Their foreign-born mothers, probably in almost all cases, expected and wanted to breast-feed their infants. The indications from the students' papers and from the mothers' comments were that the children who were not breast-fed *could* not be. There were many statements that indicated guilt or shame in the mother who could not breast-feed her infant. We may expect therefore that such mothers would often attempt to "make it up" to the child.

3. It would be remarkable indeed if adult security could be so affected by the single determinant of length of breast-feeding. We know that there are dozens and dozens of other major determinants of psychological security. But let us consider that a rejecting mother would more likely tend to wean her child earlier rather than later, to wean it quickly rather than gradually, to make breast-feeding unpleasant rather than pleasant for the infant so that it would turn away from the breast sooner rather than later, or so that it would find bottle-feeding more satisfying than breast-feeding, etc. We must also

remember that the rejecting mother, who seems to be the most important producer of insecurity, would show her rejecting attitude not only during feeding, but also in a thousand other ways, and that, furthermore, she would tend to show these attitudes in the later life of the child as well as during his first years of life.

In brief, the data become more plausible and acceptable if they are considered to be, in part, a sampling of the undoubted correlation between maternal rejection and filial insecurity and if to this are added the already mentioned indications that mothers who wish to breast-feed their babies and cannot, will, out of consequent guilt and pity, show extra love for their babies.

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AN ADDITIONAL STUDY OF FOOD AVERSIONS

BY HARRISON G. GOUGH, 1st Lt., AGD
Station Hospital, Camp Beale, California

WALLEN has analyzed the food aversions of normal and neurotic males at a Marine training station (2), and has described a list of 20 foods which can be used in studying food dislikes (3). The following brief study utilizes this list, and the general method of the former paper, in order to provide comparable findings.

PROCEDURE

A neurotic group of 79 white and colored male patients from the neuropsychiatric section of the station hospital was selected. This group included 8 mild psychoneurotics, 27 moderate psychoneurotics, 35 severe psychoneurotics, and 9 psychopathic personalities. The diagnoses were made by competent psychiatrists, in accordance with the usual criteria. A normal group of 254 soldiers who had been screened for psychiatric disorders at an Army Service Force's personnel replacement depot, prior to being sent overseas, was also selected. The normal and experimental group were adequately matched for age, race, education, Army test scores, and state of origin.

The list of 20 foods was read to each subject by one of the examiners, and aversions, as well as the reasons for dislikes, were recorded. The food list is as follows: tea, grapefruit juice, bean soup, potato soup, salmon, beef steak, veal chops, chicken, fried eggs, cottage cheese, swiss cheese, lima beans, cabbage, corn, mushrooms, radishes, tomatoes, cantaloupe, cherries, and pears.

FINDINGS

Table 1 shows the number of foods disliked by the neurotic and normal groups, in percentages.

TABLE 1

PERCENTAGE DISTRIBUTION OF NUMBER OF FOODS DISLIKED FOR NEUROTIC AND NORMAL GROUPS

No. of Foods Disliked	Percentage of Neurotics Disliking	Percentage of Normals Disliking
0	1.3	33.9
1	8.9	33.4
2	10.1	18.9
3	10.1	9.0
4	15.2	1.6
5	16.4	1.6
6	10.1	0.8
7	8.9	0.8
8	2.5	....
9	8.9	....
10	....	....
11	3.8	....
12	2.5	....
13	1.3	....

The mean number of aversions for the neurotic group was 5.14, and for the normal group 1.23, with standard deviations of 2.93 and 1.31, respectively. The difference of the means (*M* of neurotics less *M* of normals) was 3.91, and the critical ratio of the difference, 11.52. This confirms Wallen's finding of a greater incidence of food aversion in neurotic groups.

Table 2 shows the percentage of neurotics and normals disliking each food, considered separately. The difference of the two percentages in each case, the standard error of the difference, critical ratio, and number of possibilities in 100 that the observed difference is due to chance factors are also shown.

It is clear from Table 2 that the generally greater incidence of aversion in neurotics

TABLE 2

PERCENTAGE OF AVERSION FOR NEUROTICS AND NORMALS ON EACH OF 20 FOODS, WITH THE DIFFERENCE OF THE PERCENTAGES, STANDARD ERROR OF THE DIFFERENCE, CRITICAL RATIO, AND APPROXIMATE NUMBER OF POSSIBILITIES IN 100 OF OBSERVED DIFFERENCES BEING DUE TO CHANCE

FOOD	PERCENTAGE OF NEUROTICS DISLIKING	PERCENTAGE OF NORMALS DISLIKING	DIFFERENCE	STANDARD ERROR *	CRITICAL RATIO	NO. OF POSS. IN 100
Tea	29.1	9.0	20.1	4.4	4.5	less than 1
Grapefruit juice	38.0	4.3	33.7	4.2	8.0	"
Bean soup	34.2	5.5	28.7	4.2	6.8	"
Potato soup	45.6	5.1	40.5	4.6	8.9	"
Salmon	31.6	8.7	22.9	4.5	5.1	"
Beefsteak	1.3	0.0	1.3	0.7	1.8	7.0
Veal chops	20.3	2.4	17.9	3.2	5.6	less than 1
Chicken	8.9	1.2	7.7	2.2	3.5	"
Fried eggs	16.4	1.2	15.2	2.8	5.5	"
Cottage cheese	51.9	14.6	37.3	5.5	6.8	"
Swiss cheese	36.7	8.7	28.0	4.6	6.0	"
Lima beans	24.0	6.7	17.3	4.0	4.3	"
Cabbage	35.4	12.2	23.2	4.9	4.7	"
Corn	12.7	1.6	11.1	2.6	4.3	"
Mushrooms	53.2	25.2	28.0	6.5	4.3	"
Radishes	26.6	9.8	16.8	4.4	3.8	"
Tomatoes	11.4	2.4	9.0	2.7	3.4	"
Cantaloupe	13.9	3.5	10.4	3.1	3.4	"
Cherries	13.9	0.0	13.9	2.3	6.0	"
Pears	8.9	0.8	8.1	2.1	3.9	"

* Formula used as given by Arkin and Colton (1):

$$\sigma_{D\ per\ cent} = \sqrt{pq\left(\frac{1}{N_1} + \frac{1}{N_2}\right)}$$

Where: p is the total percentage of occurrence
q is $1-p$
N_1 is the number in the first sample
N_2 is the number in the second sample

remains constant when foods are considered separately.

Another aspect considered was the extent to which neurotics and normals disliked the same foods. The data in Tables 1 and 2 show that neurotics disliked more foods, and that a larger proportion of neurotics than normals disliked each food, but they do not show clearly the order of aversion for each group. The foods were arranged in descending order of aversion for both groups, and the Spearman coefficient of correlation between the two ranks was computed. This correlation was .82, with a standard error of .23. Inasmuch as *rho* is more than three times its standard error, it can be assumed that the correlation is reliable. From this it can be seen that, although neurotics dislike more foods than normals, there is considerable agreement on the particular foods disliked by both groups. Neurotics appear to dislike *more* foods, but not *different* foods, when considered as a group.

Wallen referred to characteristic reasons for aversion advanced by neurotics as another basis for discrimination. In this study there was great similarity in the reasons of both neurotics and normals; also, what might be called clinically significant explanations were found in about equal frequency in the two groups.

SUMMARY

1. An analysis of the food aversions of 254 normal and 79 neurotic male soldiers revealed

that the average number of dislikes for neurotics was 5.14, and for normals 1.23, from a list of 20 foods. The difference of the two means was statistically significant.

2. A larger percentage of neurotics disliked each food, when the foods were considered separately. These differences were also statistically significant.

3. When the foods were arranged in the order of aversion for each group, it was found that neurotics tended to dislike the same foods as normals.

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REVIEWS

EDITED BY EDNA HEIDBREDER

THE GOVERNING OF MEN. By Alexander H. Leighton. Princeton: University Press, 1945. Pp. xvi+403.

About the time this book was published the first atomic bomb exploded. There is a point in the coincidence; for, before the dust had fairly cleared, voices were asking: "How can this monster be socially controlled? What do we know about the human factor in the coming atomic era? What do we know about the governing of men in the defeated countries, or for that matter, at home?" While physical scientists were preparing the bomb in New Mexico, heedless of its social consequences, over the line in nearby Arizona Commander Leighton and a small staff were studying, on a relatively minute scale, the problem of repairing social disorganization which the crises of modern life produce. As is the rule, the zeal and expenditure for physical research outstripped the support given to social research. Yet Leighton's achievement, overshadowed though it is, marks an important event in social research.

Thanks to the farsightedness of certain public officials, including John Collier, Commissioner of Indian Affairs, Commander Leighton was assigned the task of observing and reporting on the human factor in the Japanese Relocation Center at Poston, Arizona, situated on Indian land. Confining himself to one of the units, with a population of about ten thousand evacuees, he set out to study both the disruptive and constructive forces at work in a society under stress. Part of his job was to report helpful observations to the project director. It was quickly discovered, however, that reports, even when written briefly and clearly, were of little value to a busy administrator. Much better for the social scientist to sit in policy and executive sessions to supply precisely the information and interpretation needed, at exactly the moment it is required. Even such well-disposed and "people-minded" administrators as headed the Poston project cannot take time to assimilate the findings of social scientists. The war has demonstrated in Washington as well as in Poston the useful role of the consulting social scientist on the immediate staff of important administrators.

The first two hundred pages of the report present a case-study of Poston, including the background of its uprooted population, the conflicting attitudes of the Issei, Nissei, and Kibei groups, the complex antecedent social organization that carried over into the camp even while it was disintegrating and being refashioned by traumatic new conditions. The climax of the case is the outbreak of a strike among the inhabitants whose "frustration tolerance" could not reach to the limit imposed by the upheaval they had suffered, by their discomfort in the desert heat and ill-equipped barracks, and by the darkness of their future. The strike itself, as is often the case, was focused upon specious issues. Ostensibly called for the purpose of freeing two malefactors from the post jail, actually its causes were far deeper. The role of rumors, of scapegoats, of symbols, of leadership, become clear in the course of the author's discussion of the strike.

The latter third of the volume contains generalizations that arise from the Poston experiment. Scores of "principles" are offered under the broad headings of "individuals under stress," "systems of belief under stress," and "social organization under stress." Each group of principles culminates in a list of recommendations to administrators, with special mention of points helpful to officers in military government. The book appeared just in time to assist occupation authorities in Germany and Japan (if they have time to read it).

Though broadly stated, the recommendations (like the principles on which they are based) pack a great deal of sound sense. For example, "It is not good to surround oneself with native administrative aides

whose systems of belief are merely a reflection of those in the administration"; "Expect people to respond to the administration in terms of their systems of belief—not the administration's"; "Remember that punishment is only one among many useful tools for the control of people. Never use it alone; punishment without relief of underlying stress is like tying down a safety valve and stoking up the fire."

Quoted out of context, the principles and recommendations may seem brittle and pedantic, but in the volume itself their interrelation and their dependence on circumstances are clearly set forth. Without oversimplification, the author contrives, with the aid of these generalizations, to create an orderliness in the complex field of administration. Though many well-known principles of social and psychological science are included, they have never been better articulated with concrete case material.

In speaking of the apparent variability of attitudes and opinions, the author says they "blow this way and that in the winds of circumstances, but do not readily let go of their roots." It is "systems of belief" that provide stable roots to attitudes. Since these systems behave in a dependable fashion and are resistant to change, it becomes possible to create general laws of human relations in spite of surface variations due to local conditions.

The Japanese evacuation, largely a product of war hysteria fanned into flame by the public press in California, was a major shock to its victims. The personal and social disruption was about as complete as one is likely to find, for on top of the forced migration the victims had to contend with broken promises, lack of income, inadequate supplies, poor food, and a profoundly uncertain future. The administrative task was gigantic, especially since there were no precedents to follow, and matters of jurisdiction were unclear. While some of the administrators were "people-minded," endeavoring to relieve stresses in order to rehabilitate the evacuees in the most rapid possible manner, others were "stereotype-minded," and viewed the problem as one of simple internment. "A Jap is a Jap," said they, and proceeded to treat them accordingly. The author does not spare these stereotype-minded officials whose stupidities made the task of governing the community much more difficult for all concerned, including themselves.

On the other hand, the author seems too lenient with the top administrative officers whose competence is not convincingly indicated, even though their sincerity of purpose and soundness of philosophy were clearly above reproach. It seems odd, for example, that the director should leave camp just at a time when tensions had mounted to fever height and a strike, with threatened violence, was about to break out. When he left camp he apparently did not inform the acting director of the issues at stake, and the acting director was not able to handle the situation as wisely as he otherwise might have done. In general, the reader does not sense the governing process at the higher levels of administration as keenly as he feels the stressful conditions within the unit, and the attempts of the evacuees themselves to meet their own needs. It would have been well to record more of the typical activity in the director's office in order that the reader might understand and share the author's respect for it.

From the academic point of view, the most significant feature of this book is the manner in which it smashes the barriers between disciplines. It is neither social psychology, sociology, anthropology, nor public administration; it is all four of these. In a strictly modern manner, the volume contributes to the evolving science of human relations. Trained primarily in medicine, psychiatry, and anthropology, the author does not draw his root-metaphors exclusively from these subjects. Quite the contrary, he holds his own specialties well under control, and keeps his methods and his vocabulary always subservient to his problem.

So greatly do I admire the catholicity of the author's approach that I am reluctant to criticize the specific use he makes of certain psychological conceptions, and his neglect of highly relevant psychological work. But since the new science of human relations must absorb the best portions of antecedent thought, I will indicate what seem to me

a few deficiencies on the psychological side.

For one thing, the author's treatment of "individuals under stress" would have been enriched had he cited various studies of concentration camps and of refugee life-histories which have appeared in psychological periodicals in recent years. In particular, the protective action of the ego, as brought out in Bettelheim's article, "Individual and Mass Behavior in Extreme Situations" (this JOURNAL, 1943, **38**, 417–452) is highly pertinent. Rogers' *Counseling and Psychotherapy* contains much relevant theory, and Lewin's work on group dynamics offers important exemplifications of procedures that are "people-minded."

The author occasionally slips into a popular dichotomy that social psychology teaches us to avoid. He writes, a bit tritely, that "people are more moved by appeals to the feeling man than to the rational man" (p. 361). Yet three pages later he shows a certain respect for the latter, writing, "While information and education are not cure-alls, they are powerful antidotes." His own oscillation shows that he gains little by resurrecting the old dichotomy of irrational *vs.* rational behavior. Actually the author has in his own hands the concept needed to overcome this specious distinction. Systems of belief, as he himself shows, are inextricable blends of feeling and reason, of knowledge and faith. There is no point in making an abstract distinction between their logical and their non-logical components.

The author leans heavily upon two psychological formulations that are only partly satisfactory: the frustration-aggression formula, and the law of effect. Of the two, the former is handled with greater caution. Responses other than aggression are noted on the part of the frustrated evacuees, though it seems to me that Leighton does not perceive clearly enough that planning and foresight are among the most important of all human responses to frustrating conditions.

The concepts of reward and punishment are not convincingly employed. No clear examples of the actual effects of either incentive are given. The law of effect is taken on faith. One principle (p. 284) tells us that "punishment and reward are equally important." This statement is both vague and questionable. There seems to be not a single example of deliberately administered punishment in the book, unless it be the arrest of two evacuees suspected of violence. The result of these arrests was the precipitating of the strike. The administration gained nothing, but lost a great deal, by the episode. Why, then, does the author say "reward and punishment are equally important"? Most psychological research suggests a very uneven ratio between them, especially when a human agent does the rewarding or the punishing. Reward is the more effective incentive.

Just how reward and punishment operate in social learning is, of course, a subject that lies beyond the scope of this volume. Yet there is a tendency for the author to imply that reward works somewhat on the principle of sealing wax, strengthening the rewarded channels of conduct so that they will be more readily used. Whether punishment, by contrast, is supposed to work like an ink-eradicator is not clear. But, in any case, the concepts are far too pat. Would not the author agree that people who are rewarded seldom repeat the rewarded activity, but move along to new purposes and to new means of accomplishing them? If this be the case, then it is far less important for an administrator to assign rewards and punishments as if he were Jehovah, than to see that the individuals under him are wholeheartedly active and participant in working out their respective destinies. A dog will learn a trick if rewarded with a few pieces of sugar, but a human being demands a situation where he can work out his own salvation. He will learn what he needs to learn to advance his existing interests, which is quite different from saying that he will learn what he is rewarded for doing.

These critical comments, I admit, prove little except that psychologists themselves disagree as to the essential formulations that must be included in a theory of human relations. There is no way out of this dilemma except to follow Leighton's lead: on the basis of more and more field work to obtain inductive evidence regarding the much disputed role of reward and punish-

ment, regarding the composition of systems of belief, regarding the individual's responses to stress.

One feels that the author would welcome the collaboration of psychologists in his team of observers, provided they would leave all preconceptions at the gate, and, like other participants, focus exclusively upon the phenomena of human relations as they unfold. *The Governing of Men* supplies the method, the spirit, and the initial formulations on which a new basic social science—a fusion of psychology, anthropology, sociology, and administration—can safely build.

GORDON W. ALLPORT
Harvard University

FREUD: MASTER AND FRIEND. By Hanns Sachs. Cambridge: Harvard University Press, 1944. Pp. 195.

In 1920, sensing danger in the growing popularity of psychoanalysis, Freud called together his six most trusted friends and disciples of the earlier years and formed them into "a coordinated, but strictly anonymous, group." Through private meetings and circulating letters, this group, called "The Seven Rings," was to direct the future of psychoanalysis and protect it from partisanship and personal ambition. Only two of the seven, Dr. Ernest Jones and Dr. Sachs, are alive today, and it is appropriate that one of them should record the personal impression made by Freud upon his intimate inner circle.

This book is frankly a personal record, making no pretense at full biography. It might even be called, as Dr. Sachs expresses it, "a piece of my autobiography since it concerns the personality of the man who was, and still is, a part—and certainly the most important and absorbing part—of my life." "It tells what happened under my eyes," he continues, "and describes it in the way it impressed itself on my memory; it will especially report what Freud said, when and how. Speaking in the first person I wish to appear rather as a faithful recorder than as an actor who played his part on the scene; I do not intend to go out of my way to tell about my own doings and opinions. This will be easy since the shadow cast by his personality is so much longer than my own that it covers it almost entirely and makes it invisible."

A book must be judged in the light of its author's intention. We must accept this one as a personal impression and as a piece of faithful recording; we must not look for judgments and interpretations which Dr. Sachs did not intend to include. In the recording will be found a description of the Vienna in which Freud lived, some glimpses of Freud's office and study, a few fleeting impressions of daily life in the Freud family, and a brief but vivid picture of the Nazi invasion and the departure for a few last months in England. More fully drawn are the picture of Freud as a speaker, an informal talker, and a worker; one sees a man of rare stamina who listened to patients eight or nine hours a day yet still found time to read widely, think profoundly, write clearly, and keep up a considerable correspondence in longhand. Numerous anecdotes help to enrich our scanty knowledge of this enigmatic personality. We learn of his "deep-seated aversion to showing off," his contempt for flattery, his undemonstrative nature, his rare natural gift of intuition, his intellectual independence, and a trait which "has to do with obstinacy and is not far removed from a certain form of severity; perhaps it is best expressed in the simplest words: the determination not to be fooled at any price, neither by others nor by oneself." To explain the ultimate pattern of traits—"independence, courage, and pride were the hallmarks of his character"—Dr. Sachs allots four pages to depth analysis. From scattered references in Freud's writings a case is made for the importance of the father upon whom were centered the clashing motives of love and rivalry.

In the course of Freud's life there were three notable withdrawals from his circle: those of Adler, Jung, and Rank. The noise produced by these schisms made it seem that Freud was a despot ill-naturedly driving away anyone who in the least respect disagreed with him. "It is one of the purposes of this book to destroy this legend, which has absolutely no foundation in fact." Opinions will differ as to whether Dr. Sachs

destroys the legend and leaves it without foundation in fact. Freud believed that cooperation among scientists could not be fruitful unless the participants agreed upon fundamental principles; separation was preferable to endless circular discussions which always returned to a basic point of disagreement. Moreover, Freud shunned the insignia of power and constantly tried to invest authority in some chosen follower: successively, Adler, Jung, and Rank. "This was a tactical error, since it is a well known historical fact that of all persons who are likely to get into sharp opposition to the reigning monarch, the likeliest is the crown prince." Finally, when Freud believed that anyone had lapsed from "the grace of sincerity and intellectual honesty," he reacted "not with violent reproof, but with contempt. No appeal exists against contempt, and its silence stings worse and causes more bitterness than the most outspoken condemnation. This made any further relation impossible when the breaking point was once reached."

Whatever our opinions may be on this aspect of Freud's character, we must grant that Dr. Sachs has fully carried out his announced plan of conveying a faithful record and a personal impression. It is all the more surprising, therefore, that the book is in no sense an intimate biography. One feels a tremendous distance between Freud and even one of his favorite disciples. "Freud was different," says Dr. Sachs; "some special substance had been infused into him and gave the finished product a higher degree of perfection." Elsewhere he speaks of "the gulf that separates the genius from ordinary minds," and observes that "in the bond between us something was missing—the something that leads to spontaneous intimacy between characters of similar type and tone." Reik remarks, in *From Thirty Years with Freud:* "One is not 'intimate' with a genius. . . . In conversation with me Freud was never circumspect or aloof; he was always friendly and personal—more so than ever in the last years. But the separation was too wide. There was always a barrier. My friend Dr. Hanns Sachs . . . admits that he had the same feeling in the presence of the great man." It seems plain that Freud simply did not make an intimate impression even on those who were nearest to him. Perhaps there is something in the relation of master and disciple that necessarily defeats intimacy; perhaps Freud's pride forever stood in its way. Dr. Sachs has told us what he knows, but he still leaves us puzzled.

ROBERT W. WHITE
Harvard University

THE CULTURAL BACKGROUND OF PERSONALITY. By Ralph Linton. New York: D. Appleton-Century, 1945. Pp. xix+157.

This excellent little book stemmed from five lectures on *Culture, Society* and the *Individual* which Professor Linton delivered at Swarthmore College in 1943.

By intent (*vide* the cryptic dedication) and in fact, these five short chapters constitute a challenge to the academic clannishness of vested departmental interests. But the book is not a mere polemic for unity among social scientists in the study of man. It consists of a cogent survey of areas usually allocated to cultural anthropology, but merges this with an intellectual invasion of ground over which psychologists have proprietary feelings. If I am any judge, the result is a decisive victory for Linton's cross-departmental approach.

Three chapters bearing distinctly anthropological titles, "The Individual, Culture and Society," "The Concept of Culture," and "Social Structure and Cultural Participation," are neatly informative. They will perhaps prove reassuring to those psychologists who have sometimes looked askance and sometimes with envy at the readiness with which some cultural anthropologists render generalizations upon human collectivities. These chapters, in addition to many useful analytical definitions and distinctions as between *culture* and *society, overt* and *covert* culture, *ideal* and *real* culture patterns, include a critical discussion of anthropological methods of observation and reporting. In the matter of selecting informants to study an alien culture, the sampling problem is clearly recognized and a proposal made for independent study by several investigators as a possible check on the validity of field studies.

More important is Linton's formulation of the "culture construct," a term introduced

to tag anthropologists' abstractions from observed behaviors. The modal quality of such "culture patterns" is clearly stated.

> In order to present a comprehensible picture of any culture, or to manipulate cultural data, the investigator has to develop a *culture construct*. He establishes the mode of the finite series of variations which are included within each of the real culture patterns and then uses this mode as a symbol for the real culture pattern. (p. 45)

The intellectual legitimacy of such mental operations should be challenged by none.

The two remaining chapters in this book are concerned with "Personality," and "The Role of Culture in Personality Formation." These contain an elaboration of a need theory of motivation in which a need for "emotional response from other individuals" (p. 7) is described as man's most outstanding and continuously operative need. This, along with a need for long-term security, and a need for novelty, is given the role of prime mover in human behavior. Provisions for fusion of needs and recognition of their varying urgency are made. Needless to say, Linton gives adequate attention to the role of the milieu in shaping personality. Not only is the total social environment credited with stamping all members of a given society with a "basic personality type," but stratified segments of a social organization are viewed as productive of "status personalities"—an interesting concept. Also in these chapters a learning theory, replete with a discussion of trial and error and imitation, is presented which seems actually to gain in realism by its freedom from the artificiality (ratificiality) of the laboratory. (And herein lies a moral for psychologists gleaned, *passim*, from the book.) As experimentation is not the goal of science, but properly the means to knowledge, expressed in predictive theories, so psychologists must go beyond tentative and timorous statements of theory attached to the beginning and ending of their neat little experiments. A little boldness in the grinding of cortices as opposed to perseverance in the punching of calculators or IBM cards will result advantageously if Linton's book is any model. For this little book is richer in theories describing the whole man in his milieu than many psychological volumes with twice its pagination.

The book is, in sum, an intellectual tonic for those who feel bound by the traditional fare of psychological textbooks. An enviable felicity of expression marks the writing.

Some work of Professor Lewin and collaborators makes it possible to question Linton's opening statement that the intrinsic qualities of cultures and societies are such that it is impossible to produce them experimentally.

JOHN ARSENIAN
Center for Group Dynamics
Massachusetts Institute of Technology

PRACTICAL APPLICATIONS OF DEMOCRACY. By George B. de Huszar. New York: Harper and Brothers, 1945. Pp. 140. $2.00.

Although written primarily for the layman, this little book will interest social psychologists because it coincides with a current development in the social sciences. The establishment of the Research Center for Group Dynamics at M.I.T., the formation of the Commission on Community Interrelations, the publication of *Social Issues* as a nontechnical journal of the SPSSI, the rapid increase in the American Association for the Study of Group Work, are all indicative of the direction in which social psychology is moving. Social scientists are doing significant research on current problems, and, at the same time, they are accepting their responsibility for translating research findings into action by working cooperatively with community or agency leaders at the point where the problem is felt. The work of social scientists in industrial organizations, governmental agencies, social organizations such as the Boy Scouts, the USO, the Jewish Welfare Board, etc., bears witness to the transfer of the laboratory from the cloistered tower to the work-a-day world.

In the opening paragraph of his preface, Mr. Huszar states, "Democracy is something you do; not something you talk about. It is more than a form of government, or an attitude or opinion. It is participation." Although not presented here, evidence which supports the theme of the book is accumulating from a variety of sources.

The author contends that there is an increasing tendency toward centralization and authoritarianism in modern life which is accompanied by social disintegration and by inaction on the part of the citizenry. He sees danger in the willingness of people to sacrifice their freedom for security. Thus, he poses the problem of democracy as how to develop social integration resulting from participation and action by the members of society.

Starting from the thesis that the most important groups are primary groups, which are characterized by face-to-face relationships, Mr. Huszar develops the use of problem-centered, study-action groups as the method needed to put social control into the hands of the people. After discussion of the method of problem-centered groups, a number of applications are described. A chapter each is devoted to the application of the method to community life, government, journalism, industry, employment and vocational training, and education, art, and leisure.

The concluding section considers the effect of group-participation on the individual. Mr. Huszar contends that the development of problem-centered groups not only will revitalize democracy and make for beneficial social change but will also serve a mental hygiene function for the individual participants.

There is no doubt that this book deserves a widespread circulation. In the midst of big business, big labor, big government, big pressure groups of all kinds, the emphasis upon what the individual can do is refreshing and badly needed. "A middle way must be found, which does away with the dichotomy between rugged individualism and collectivism. Related individualism, which views individuals as the result of relations with one another, is such a middle way." "There is no way of modifying the shape of the social world except by attending to concrete situations which are under our control, which we can affect." Certainly the person who takes Mr. Huszar's book to heart should be encouraged to find that his possibilities of control over his and his society's future are greater than he has realized. His participation need not be limited to voting when required, writing letters to congressmen, signing petitions, joining giant organizations, etc. As the author states it, "The question is not that of abandoning consent-democracy . . ., but that of supplementing and combining consent-democracy with do-democracy."

With a thesis as stimulating as his book presents, Mr. Huszar can be forgiven if his enthusiasm has carried him to extremes. There is no consideration of the disruptive factors within small groups. The discussion of group-structure and of factors which affect it is far too limited. The importance of leaders skilled in group-work techniques for developing democratic participation is ignored. Further, the author's faith in group activity *per se*, without regard to the *quality* of the group experience as a therapeutic agent seems overly optimistic. An unfortunate group experience can also be traumatic. The result of these deficiencies is that the reader turns from the book with the feeling that the formation of small, problem-centered groups will, of itself, solve the problem of effective social action. This conclusion follows from the implicit assumption that all members of problem-centered groups are equally capable of utilizing the group experience. However, skilled leadership is often necessary to prevent a small group from becoming fatally disrupted as a consequence of emotional clashes among the members. The crucial part played by the attitudes, opinions, and personal idiosyncrasies of the group members is completely neglected. This failing of the book could have been remedied if verbatim case studies of meetings of small groups could have been included. The reader could then have seen the effect of non-rational factors. Further, analysis of such verbatim case studies could have pointed up a number of leadership skills which are necessary if a truly democratic group-atmosphere is to be established and maintained. However, when viewed in the light of Mr. Huszar's aim of talking with the average citizen, these deficiencies would not appear to be too serious.

Another question which has been left untouched concerns the kinds of problems upon which groups will be allowed to work.

Who decides what will be the problem at the center of a group? This difficulty arises when Mr. Huszar shifts from considering problem-centered groups as spontaneously formed by the members (e.g., community groups) to groups formed by management as a personnel procedure (e.g., industry and education). If a group is originated by its members, obviously there is no limitation on the kinds of problems they can attempt to handle. The use of group-decision as a personnel procedure, however, poses a different situation. When the question is raised as to what kinds of problems student-groups will be allowed to solve, or as to what decisions industrial employees will be allowed to make, the basic issue becomes apparent. For example, Mr. Huszar gives many instances of groups of industrial employees devising methods of production, changes in equipment, uses for waste materials, etc., but he does not consider whether employees should also be allowed to participate in determining the manner of distribution of industrial profits. If the group is allowed to handle only certain kinds of problems, what is the effect upon the morale of the members? Is there any possibility that group-decisions on minor matters could be used as a technique of exploitation of workers? Perhaps the issues are too explosive for discussion, but it seems clear that psychologists concerned with the psychological problems of democratic action cannot ignore the question of areas of responsibility. The very nature of the subject matter of physics may lead physicists to develop devices such as the atomic bomb without regard to the consequences upon society, but it does not seem that psychologists share the same immunity from the social implications of their research. One by-product, then, of reading Mr. Huszar's book might be a challenge to the psychologist to define his function in modern society.

The significance for psychologists of *Practical Applications of Democracy* would seem to be at least three-fold. First, the various deficiencies of the book are all related to problems needing more research. If small groups are such effective units of action, then certainly we need to know much more about the dynamics of such groups and about techniques of training effective democratic leaders. Secondly, perhaps the teaching of psychology should have something to do with the fulfilling of Mr. Huszar's implicit assumption that all members of problem-centered groups are equally ready for effective group experience. Has psychology yet reached the stage of development where we can teach for outcomes that improve the social living and the self-development of the students? There is an increasing number of psychologists who think so. Certainly any elementary psychology class can be a laboratory in human interrelations. Thirdly, perhaps some psychologists who read this book will organize problem-centered groups in their own communities (as well as in their professional societies and in their classrooms) and thus become democratic participants themselves.

JOHN L. WALLEN
Black Mountain College

SOCIAL-ECONOMIC MOVEMENTS. By Harry W. Laidler. New York: Thomas Y. Crowell Co., 1945. Pp. xx+828.

Dr. Laidler's work is conducive and perhaps indispensable to an adequate historical comprehension of social struggle. The present volume grew out of his earlier study, *A History of Socialist Thought* (1927). Parts of it form a rewritten version of the older book, which is now made one-third larger by the addition of twelve wholly new chapters. The added material covers the recent development of cooperatives, the Russian scheme of life, and the general progress of social democracy, hampered as it has been by totalitarianism and war, to 1944.

While the presentation is "straight history" that makes no direct psychological contribution as such, it contains numerous items of implicit concern to the social psychologist. Here is the record, since the Middle Ages, of man's inability to devise a half-way satisfactory or approximately moral mode of social existence. Here is the detailed evidence of the consistent triumph of individualistic greed and irresponsible political tyranny over the creative aspirations and life needs of the rank-and-file of mankind. Here is, in effect, a portrayal of chronic psychological disintegration on a collective scale

to which there is nothing comparable among individual organisms in the entire annals of medical research. Society's diseases ravage virtually unchecked despite the clarity of diagnosis and the growth in technical competence on the part of society's volunteer physicians—while somehow the patient lives on, after a fashion!

As a competent historian Dr. Laidler avoids making the easy over-simplifications found in merely doctrinaire protests. The indicated heroes and villains of the piece are not cartoon characters, nor are they for the most part personified at all. One obtains rather the impression of an essentially uncontrolled drift of *homo occidentalis* in the direction of self-frustration. The participation of the traditional agents in the drift, such as finance capitalism, mass production, nationalism, and competitive conflict, usually present as tyranny of the "right," is shown to be subordinate to basic inadequacies of human outlook which unfortunately remain unanalyzed. The fact that the author does not grapple with the psychological fundamentals of the total process is no reproach to him, considering the aim of his work, but it reveals that the title of the book is, to say the least, misleading. Apart from disappointed hopes of anaytical social scientists, "social-economic movements" properly include reactionary as well as progressive, rightist as well as leftist developments, but the book is actually "an historical and comparative survey of socialism, communism, cooperation, utopianism; and other systems of reform and reconstruction."

Of interest to the psychologist should be the striking fact, often helpfully illuminated by biographical material, that the minority of "socialistically" inclined thinkers has been recruited from the population at large, comprising all sorts and conditions of men, and not from personalities specialized to some distinctive maladjustment. The seekers after social and economic justice whose contributions and influence are summarized by Dr. Laidler appear to be representative of all the various biological, social, economic, and psychological classifications of mankind. Because they do not run to any consistent type their limited number in any generation is a problem for psychology to consider. Debs and Tolstoi, Fourier and Mill, Owen and Croce, Thomas and Trotzky, brought into juxtaposition as personalities, illustrate diversity more than anything else. But since the club is not exclusive, why has it had so few members?

This question raises a more embarrassing one: When nineteenth- and twentieth-century stresses brought into the circle of liberal social and political thought economists, biologists, sociologists, philosophers, and even mathematicians, *where were the psychologists?* Social psychology is as old as Marx's great work, and experimental methods were introduced not long after it appeared. Yet the overlapping of persons in Laidler's history with those in histories of psychology is confined to the *first* half of the nineteenth century. The only outstanding exception is the case of John Stuart Mill, and his psychological activity likewise belonged to the earlier period. Between the romantic formulations of the Utopians and a few very recent contributions to social psychology (by Russians, the late Theodor Lessing, students of public opinion, etc.), virtually no leftist social theorizing was accomplished on the basis of hypotheses about the constitution and functioning of human nature. Fourier is one minor exception to this statement. The leading lights of nineteenth-century psychology. Ward, Spencer, Bain, Ribot, James, the whole brilliant galaxy of German psychologists except such men as Brentano, and most of the better-equipped psychologists of the present century, seem to have been neither present nor voting. Doubtless the demand for brass-instrument techniques and the self-conscious urge to defend psychology-as-a-natural-science account for the situation, but it is nevertheless a little shocking. All the while, numerous *philosophers* who started out in normal academic patterns readily applied their insights to the problems of society. One may recall that Karl Marx's doctoral dissertation, in philosophy, was on the "Natural philosophies of Democritus and Epicurus."

On the other hand, despite his thorough combing of modern history for thinkers who were not social reactionaries, Dr. Laidler omits mention of names one would expect to encounter. There is no reference to Max

Nordau, who had considerable popular influence, nor to Adin Ballou, who achieved relatively greater success than most communal pioneers in establishing his Hopedale Community, nor to Emanuel Swedenborg, who in the words of another unmentionad participant, the elder Henry James, viewed "society as the redeemed form of man," and who portrayed a social order based on spiritual regeneration that is remarkable for its sophisticated psychological grounding and that deserves inclusion along with St. Augustine's *Civitas Dei*.

While Dr. Laidler's statistical demonstration of the growth of social democracy in most western countries except the United States reveals not inconsiderable progress up to and since the outbreak of the present war, his presentation seems in general to suggest that since the defeat of the most promising and flexible revolutionary efforts of just a century ago, socialistic advance may have become a lost cause because of fundamental changes in the pattern of man's outlook. Socialist principles may yet win out, as he suggests, but the framework of the struggle for their establishment continues rapidly to change its form. Here perhaps is the master-problem for psychology, if one applies to the historical struggle the concepts of the "configurational" school of anthropology. As Norbert Elias has indicated,[1] the deeper dynamics of social life may be essentially a matter of *attitudes* on the part of those of a given culture, and just as a basic disruption of medieval integrations preceded the "modern period," so a similar change in the cultural pattern of Western civilization (with or without the intervention of new dark ages) may be now in progress. A structural shift of attitudes, motives, norms, and values, is surely of vast psychological significance.

HOWARD DAVIS SPOERL

Jeffersonville, Vermont

PSYCHOLOGY OF RELIGION. By Paul E. Johnson. New York and Nashville: Abingdon-Cokesbury Press, 1945. Pp. 288.

Consistently treated as a poor relation or orphan of scientific psychology, the field of the psychology of religion flourished a generation ago but has been little cultivated since, especially in view of the overshadowing growth to relative maturity on the part of social psychology and its offshoots. It is therefore an event of importance when any "psychology of religion" makes an appearance; the importance is enhanced if, as in the present instance, such a work shows signs of being a new, comprehensive, and up-to-date treatment of the field.

The "poor relation" status is rapidly being subjected to revision because of the combined impact of personality theory, developmental psychology, and depth psychology upon a growing concern with the psychological aspects of social problems. One such aspect, which even the stuffiest operationist must(?) admit "will not down," is the religious. The effect of the combined impact of the disciplines mentioned is to provide a personal and interpersonal context in which the role of religion becomes increasingly significant, purposeful, and valuable. If it was primarily the opening of the field of personality that did the trick, one may see justice in the reflection that the term personality came into psychology from the philosophy of religion.

As Professor Johnson suggests, *personality* has come to signify in one respect the psychologist's "equivalent of the 'soul'," that discredited abstraction which musty theologians think of as pervading individuality and of being subject to growth, transformation, regeneration, and "salvation." Now that non-theologians have taken to honoring similar processes, even to that of "re-structuring the self in outgrowing one's egocentricity," it might be permissible to remind them that religious discipline was ever replete with methods of salvation from self-love, whether referred to as "original sin," "the old Adam," "depravity," etc., or in the new fashion as "egocentricity," the "pleasure principle," or "primary narcissism."

At any rate, the psychology of religion as it is written in 1945 is no hodge-podge of special topics that resist ready assimilation into Regular Psychology, perhaps infused with a touch of clinical apologetics for bringing them up. It is rather an account of the development and functioning of the person, with special respect to his value experiences

[1] See this JOURNAL, 1941, 36, p. 298.

and his value determinations in behavior. The time-honored chapters on the origin and authenticity of religion are compressed into a few pointed paragraphs. The one-time "extremists," formerly regarded as dubious enthusiasts, James and Hall, are placed in a by no means negligible focus while their historical precocity finds ready interpretation in the perspective of personality theory as we now know it. Were the reviewer still immersed in an atmosphere of down-country vulgarity, he might be tempted to say that the psychology of religion has become "streamlined."

Professor Johnson is at his best in the first five of the ten chapters that make up the book. His treatment of religious growth and regenerative powers is a brilliant integrating of the best findings of developmental psychology with what we are not ashamed to call, without quotation marks, the spiritual life. Unfortunately, after dealing in a concisely informative manner with the theme of conversion, he shifts to a different ground on turning to prayer, worship, belief, and religious behavior. All of these topics surely represent aspects of regeneration, and they can be given greater point and closer coupling by being treated as such. The oldest complete psychology of religion in modern times (which is omitted from Johnson's historical section, although he mentions Jonathan Edwards and Schleiermacher) was Swedenborg's, and it never lost sight of the unifying regenerative process, portrayed in not un-Allportian terms much as the latter's "transformation of motives" in perpetuity.

Diffuseness of treatment is otherwise frequently manifested in the book through the quite unnecessary introduction of miscellaneous collateral contributions. There is a list of McDougall's instincts, one of Murray's needs; there are comparisons of Freud's views with those of his pupils; Leuba, the official whipping-boy of the field, is digressively chastised; and throughout there are evidences of a mania for collecting other similar theoretical side-shows. There is too much needless terminology, and altogether too much free advertising of the irrelevant views of numerous investigators. One cannot decide whether this over-inclusion is due to the author's Christian spirit ("coals-of-fire-hypothesis") or to a desire to use in some way his superfluous reading notes *(Sättigungsdrang)*. A possible value here is to be seen in the opportunity to enlighten psychologically the non-psychologist readers such as ministers; this value is destroyed, however, by the fact that the side-shows are little more than a superficial cataloguing.

The concluding chapters, which purport to show the bearing of the psychology of religion, as previously presented, on the church and the community, "gyre and gimble" to a considerable extent. This is because the author is making a manful effort to interpret *existing* churches as religious institutions, when his forthright psychological interpretation of religion as a personal and social force has already clearly implied that they are nothing of the kind (to a greater or lesser extent). Logicians ought to find a name for the fallacy, here represented, of attributing to the world the best qualities of a process that exists only because the world lacks those qualities. The psychologically informed and spiritually sensitive reader can easily appreciate and applaud Professor Johnson's portrayal of religion in its psychological bearings; when the assertion is attempted that *this* religion is what the *church* happily receives as religion, there is trouble. The point, needless to say, applies to the *Protestant* church and culture which the author primarily has in mind.

There are too many errors in the book for comfort, such as crediting Wheatstone with the invention of the chronoscope, referring (as freshman psychology students do) to the "intelligent quotient," and sometimes using what appears to be a different word from that intended. The bibliography is extensive, documentation is thorough; no references go beyond sources in English (which cuts out some fairly important recent work); and any *gottlose* psychologist can afford to buy the book (do him good).

HOWARD DAVIS SPOERL
Jeffersonville, Vermont

THE HUMAN MIND. (Third Edition.) By Karl A. Menninger. New York: Knopf, 1945. Pp. xvii+517. $5.00.

This latest revision of Karl Menninger's

best-known work seems to be a revision in name only. Its chief departure from the text which appeared eight years ago consists of a newly written section in which the author purports to do justice to "new developments in therapy" within a space of seven pages (pp. 410–416). In this pin-point account of a decade of progress in psychological medicine, Dr. Menninger devotes six lines to the engrossing and timely subject of military psychiatry (p. 416).[1] By and large, however, the text stands pretty much as it was originally issued fifteen years ago. As he reveals himself in this little-revised edition of his major opus and in his contemporary journal publications, therefore, Menninger at fifty-two remains in essence one of the purest of the purer and more important American Freudians.[2] This continued state of orthodoxy may account for the fact that the present text retains, so faithfully, its original form. Perhaps the author says nothing new for the reason that he has nothing new to say.

In passing so harsh a judgment, one must in all fairness acknowledge the positive side of Dr. Menninger's activity, past and present. When it first appeared, in 1930, *The Human Mind* was a first-rate addition to mental hygiene literature. It helped to popularize psychodynamic concepts of medicine in a day when these concepts were relatively new to the layman and the medical profession itself.[3] The book's subsequent wide circulation likewise stamped the author as a successful pioneer with a relatively untried literary form. Blessed with rather unusual expository gifts, Menninger demonstrated the fact that a scientist could present his ideas in more or less popular form, without sacrificing either scientific status or professional dignity. He succeeded in this effort—it should be remembered—at a time when most American scientists, and psychologists in particular, were still self-centered in their habits of publication, writing, as it were, monotonously and with private vocabularies, for one another. The Menninger text has been enormously popular. To date it has sold upwards of 200,000 copies.

These and other very real achievements do not, however, guarantee for the current edition of Dr. Menninger's popular text a valid passport to the present or the future. In fact, the lesson of the third edition of *The Human Mind* would seem to be that anyone who dares remain a Freudian fundamentalist in 1945 is taking his life in his hands, so to speak.

By hewing in 1945, as he hewed in 1930, to the tenets of a Freudian orthodoxy, Menninger overlooks what would appear, to this reviewer, to be the past decade's most important deviations from that doctrine. He makes no mention whatever of the American Rankians (such as Taft and Robinson in the field of social work or Dr. Frederick H. Allen in the realm of psychotherapy with children) who have posed a challenge that goes to the very root of Freudian theory and practice. Nor does the reviser of *The Human Mind* so much as refer to the promising experimental (and neo-Rankian?) program which Carl Rogers initiated at Ohio State University. Dr. Menninger fails to sense, by the same token, the rise of a new profession of psychological counselors and lay therapists (a good proportion of whom will, of necessity, do their work in conjunction with psychiatrists and other medical practitioners).[4]

Several of the author's direct references to academic psychology—observations that are

[1] The revision was completed in November, 1944. By that date the neuropsychiatric procedures of the Allied armies had been reported in a fairly extensive literature, some of which came from the pen of the author's distinguished brother, Col. William C. Menninger, chief of the Neuropsychiatric Division of the Surgeon-General's Office in Washington.

[2] Outside the field of psychotherapy, however, both of the Menningers are eclectics and indefatigable research workers.

[3] This healthy influence on the medical profession continues even more effectively today, through the manifold activities of the Menninger Foundation in Topeka.

[4] By way of comment on an estimate that the United States could use ten thousand more psychiatrists than it now has, Menninger does make the prediction that medicine will soon have to avail itself of the services of a larger number of "trained aides," such as social workers and clinical psychologists. But in making this forecast he in no way relaxes his Freudian principles. And despite the fact that the work of Rogers and others seems to signify that academic psychology is, at long last, moving into the clinical field with vigor and a mind of its own, the clinical psychologist whom Menninger envisons in the future will be little more than a mental tester, to be called in at the point of diagnosis.

carried forward from the first edition—might well have been revised. On page 176 he still holds to his earlier opinion that psychologists in half a century of effort have not "greatly improved" on the work of Simon and Binet. He also repeats the charge that the academic psychologist "is (or has been)" preoccupied with the study of the human mind "shut up in a laboratory" (p. 12). The second criticism was a just one when it was first raised, fifteen years ago.

It is in his social thinking or in his broader applications of psychiatric theory that this talented and influential Freudian disciple pays the heaviest price for his fundamentalism. Inasmuch as his present revision differs so slightly from the original text, he would apparently offer it to the reader as a sufficient guide to the contemporary scene. But the key which promised to force all doors in the twenties—the libido theory and its sundry corollaries—simply fails to fit a good many of the locks of 1945. Without meaning to do so, Menninger thus makes it abundantly clear that no scientist, psychiatrist or otherwise, can do justice to the total problem of human adjustment if he falls short of understanding that many of the emotional difficulties which beset "the human mind" are culturally induced or are a product of the particular set of conditions under which we live.

Still tightly clutching his little bundle of Freudian instincts, the noted author of *The Human Mind* takes no more than passing note of the culture which the anthropologist and the social psychologist, and millions of discerning laymen, have been turning inside out for the past ten years or more. If one may judge from his apparent lack of sociological orientation, Dr. Menninger would probably not agree in any thoroughgoing manner with Brig. John Rawlings Rees, the consulting psychiatrist to the British Army, who said recently that the need of the hour is not so much one for more and better clinicians (though that is important in itself), but rather a need for a larger number of well-grounded "medical sociologists."

In the new edition of *The Human Mind*, as in the old, we learn that war and racism—two human activities which Dr. Menninger abhors—are largely expressions of the death instinct, of our unruly "destructive impulses," of man's everlasting desire to destroy himself (pp. 210, 486). This, despite what the experts have told us (Hadley Cantril, Frederick L. Schuman, the recent contributors to the new SPSSI journal, *et al.*) and what a good many laymen already suspect about the psychodynamics of such social pathology: that racism and war recur not as an expression of a set of mysterious and uncontrollable instincts, but within certain known, and preventable, social contexts.

Finally, though he is clearly a man of good will and though he earnestly enjoins his medical brethren to come down from their ivory towers,[8] Dr. Menninger still becomes, on occasion, deeply pessimistic. For a thoroughgoing Freudian, who stresses, among other things, the inherently bad in human nature, and whose preoccupation with purely psychic and biological forces leaves him with explanatory categories that are not equal to the situation in which the world finds itself today, such a retreat is understandable.

The author's gloomy outlook may be illustrated by his closing remarks on the subject of criminology. After years of gallant but—in his eyes—more or less fruitless agitation in behalf of an enlightened criminal code, Dr. Menninger lost heart some years ago. That battle, he writes in 1945 as he wrote in 1937, is seemingly lost, at least as long as we retain "the barbarism of our present ideology" (p. 456). As a piece of social diagnosis, this counsel of despair and futility would appear to fall as wide of the mark as the well-intentioned general practitioner of the old school who continues to tell his neurotic patients to go home and forget their troubles.

KEITH SWARD

Beverly Hills, California

THE SOCIAL SYSTEMS OF AMERICAN ETHNIC GROUPS. By W. Lloyd Warner and Leo Srole. New Haven: Yale University Press, 1945. Pp. xii+318.

This third volume of the Yankee City Series is a most interesting study of the social systems of ethnic groups in an extant New

[8] Even this sincere call-to-action is couched in libidinous terms. The doctor who participates in the social struggle, Menninger repeats, will literally save his own life; he will save himself from his own destructive tendencies by concentrating on the destructive impulses of others (p. 486).

England community. It evoked two impressions of interest to potential readers: First, the book may profitably be read independently of the first two volumes. Secondly, the authors admirably succeed in presenting graphic pictures of the conditions and positions of eight ethnic groups in the community. In order of their numerousness—although it is repeatedly said that order of arrival in Yankee City is a much more significant datum—these ethnic groups are Irish, French Canadians, Poles, Greeks, Jews, Italians, Armenians, and Russians.

In all, there are ten chapters detailing respectively the spatial distribution of the groups, their economic participation in the community, their positions in the social hierarchy, their family systems, church affiliations, memberships in associations, language behavior, and schooling. Three additional chapters are assorted in content. The first presents some anecdotal material in dramatic form illustrating problems confronting the Irish group in assimilating to the dominant Yankee culture. Although it is asserted that "Essentially the problems of the Irish are the same as those of the other people with divergent cultural traditions," this writer privately doubts whether the Irish can properly be regarded as representative of any but the children of Erin. However alike the assimilative problems, those having a penchant for gossip will miss companion chapters for the other ethnics. One could cavil with the title, "The Melting Pot; Seven Personal Histories," of this first chapter. Each has about as much of "personal history" as befalls characters in the opening paragraphs of a novel of social manners. Another chapter, titled "Methods for the Study of Ethnic Groups," might similarly elicit the small criticism that this is too promising a heading to spend upon three pages in which an apt classification of generations is the sole methodological feature.

A final chapter gives something of theoretical adequacy to a book otherwise loaded with excellent descriptive material on ethnic groups in Yankee City. It contains a bold conceptual scheme categorizing by scales several salient traits of ethnic groups in America—not merely Yankee City. From these come predictions regarding time required for assimilation to the dominant culture by ethnics ranging in their positions of relative subordination from immigrant English to West African Negroes.

The underlying problem of the book is posed as "an examination of the validity of America's conception of itself as the 'great melting pot'." Implicitly the entire volume spoils the analogy as Anglo-Saxon protestantism seems to compose the mould of the pot. Although this volume tells us little about the mould, it describes very satisfactorily the ingredients in the pot and what changes they have undergone thus far in the melting process.

JOHN ARSENIAN
Center for Group Dynamics
Massachusetts Institute of Technology

THE STORY OF THE SPRINGFIELD PLAN. By Clemence Chatto and Alice L. Halligan. Introduction by Clyde C. Miller. New York: Barnes and Noble, 1945. Pp. xviii+201. $2.75.

In 1939, the superintendent of the Springfield, Massachusetts, schools appointed a committee. Teachers, principals, and supervisors from all levels of the schools were its members. They were selected apparently on the basis of their strong interest in what they were to do: to prepare plans for the extension and development of work in intergroup and citizenship education already begun by some individual teachers. *The Story of the Springfield Plan* is a report to the general public of how, during five years, the plans were worked out and put into effect. The story is told with pride, modesty, and enthusiasm.

Reported are the methods and rules governing the employment and advancement of teachers; the methods used in deciding upon and installing new teaching procedures and new curricula; specific methods, content, and social arrangements used; efforts to achieve better intergroup relationships and citizen behavior among adults. In an appendix, the superintendent, Dr. John Granrud, presents a realistic and enlightened statement on "The Democratic Administration of a School System." Throughout, there has been courage, persistence, and resourcefulness in the application of social-psychological knowledge to

minimize insecurity and aggression, to maximize cooperation and mutual understanding, and to promote equality of opportunity and to recognize merit in whomever it is found. That the greatest success so far has been achieved with the younger age group is not surprising.

If the atmosphere of the book reflects at all accurately the social atmosphere of the Springfield school system and community, a great deal has been achieved in actualizing our democratic ideals. The evaluation of the program is presented largely in terms of illustrative instances of desirable behavior patterns and in terms of the opinions of prominent citizens. Systematic and quantitative evidence of changed attitudes and behavior are, however, almost entirely lacking. If such evidence has been secured, it is unfortunate that it has not been presented.

STANLEY G. ESTES

Harvard University

NEW BOOKS RECEIVED

November 1, 1945

FENICHEL, OTTO, *et al.* (Eds). *The psychoanalytic study of the child.* New York: International Universities Press, 1945. Pp. 423. Price $6.00.

GRINKER, ROY R. *Rorschach's test: II. A variety of personality pictures.* New York: Grune & Stratton, 1945. Pp. xii+402. Price $5.00.

JOHNSON, PAUL E. *Psychology of religion.* New York and Nashville: Abingdon-Cokesbury Press, 1945. Pp. 288.

LOWY, SAMUEL. *New directions in psychology.* New York: Emerson Books, Inc., 1945. Pp. xiv+194. Price $3.00.

ROHEIM, GEZA. *The eternal ones of the dream.* New York: International Universities Press, 1945. Pp. xiii+270. Price $4.50.

WELLS, F. L., AND RUESCH, J. *Mental examiners' handbook.* New York: Psychological Corporation, 1945. Pp. 211.